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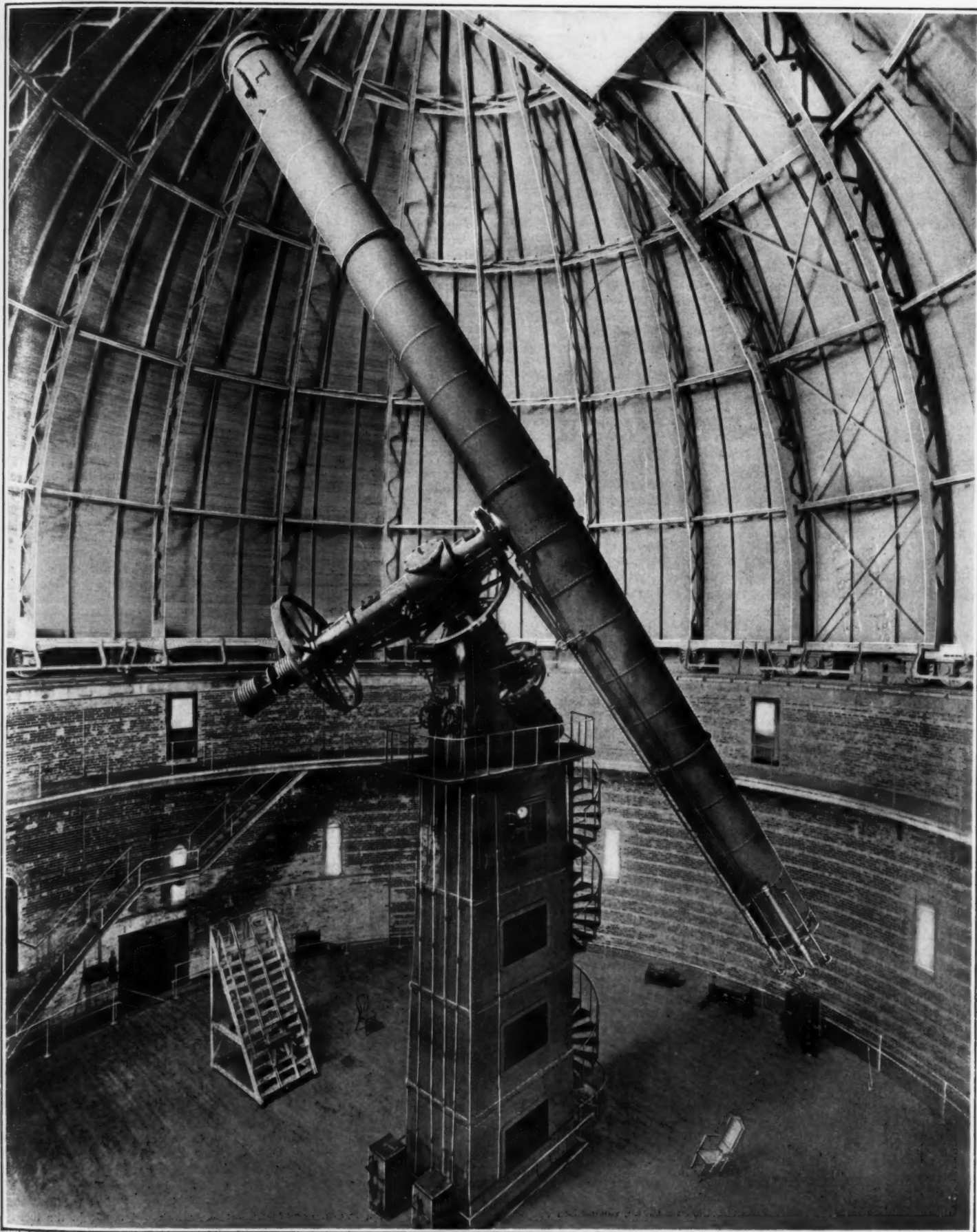
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The great forty-inch refractor at the Yerkes Observatory, with adjustable floor nearly at its lowest point.

TWO IMPORTANT AMERICAN OBSERVATORIES.—[See page 72.]

Evolution and Mendelism*

The Responses of Organism to Changes in Stimulation

By R. Broom, D.Sc.

THE appearance of Darwin's "Origin of Species" fifty-six years ago is generally admitted to have been the most important event in the history of biology. Though others before him had believed in the evolution of living forms, it was not till Darwin had brought together and arranged his wondrous wealth of facts that the scientific world was convinced that whatever the cause or causes of evolution there was no longer any doubt as to the fact. Wallace and Darwin further advanced the very plausible theory that the natural selection of the varieties best fitted to survive in the struggle for existence was the chief factor in the evolution.

Many, perhaps the majority of biologists, accepted this theory of Wallace and Darwin as giving a satisfactory explanation; others, while accepting the truth of evolution, felt that though natural selection had undoubtedly played a part, there must have been some other agency. Of these latter Cope was one of the most prominent, and in his book "The Origin of the Fittest" he endeavors to get at the causes of the fitness that is selected.

In the last thirty years there has been very little advance made in our knowledge of the causes of evolution, but our knowledge of the facts of biology has increased enormously.

The embryological history of most of the principal living types is now fairly well known; while paleontology can now give a moderately satisfactory view of the types of animals, and to a less degree of the plants, which flourished in the various geological epochs.

New fields have been opened up by experimental embryology, and the minute study of the animal cell and the changes that take place during fertilization and cell division has greatly added to our knowledge.

Since the beginning of the twentieth century, perhaps the most important biological work that has been done has been that of the disciples of Mendel, who by cross breeding closely allied varieties of animals and plants have shown how parental characters are rearranged in the descendants. The Mendellians have undoubtedly thrown more light on the nature of heredity than all the earlier investigators together, and the facts they have revealed are of the utmost importance not only to the horticulturist and the stock breeder, but in showing how it may be possible to eliminate certain defects, and foster desirable qualities in the human species.

In his addresses as president of the British Association at Melbourne and Sydney in 1914, Prof. Bateson reviewed some of the more striking results obtained by the Mendellians, and the bearing of the discoveries on human progress. The Melbourne address was devoted mainly to the relations of Mendelian facts to evolutionary theories, and many of the statements made are so startling and so opposed to views that have been very largely held in the past that one feels somewhat bewildered.

As is well known, the Mendellians, by cross breeding two varieties of a species which differ in regard to a certain character which is being studied, find that though the character may not be manifest in the resulting offspring it reappears in a certain proportion of the next generation; and further that a definite proportion of this last generation breed true as regards the characters of the original parents experimented on. From this it is assumed that the characters of any plant or animal are due to certain genetic factors which are present in the germ cells, and that if the form could reproduce itself asexually or if the two sexes were perfectly similar as regards their genetic factors, each generation would be like the previous one.

It is accepted as an "essential principle, that an organism cannot pass on to offspring a factor which it did not itself receive in fertilization," and also that "parents which are both destitute of a given factor can only produce offspring equally destitute of it." How then, it may be asked, can new forms arise? On this point there does not seem to be complete agreement among Mendellians. Lotky believes that all new forms are the result of crossing, and even goes the length of suggesting that the first vertebrate arose from the crossing of two invertebrates. Bateson, while he does not disguise his sympathy with Lotky, believes that new forms may arise by the releasing of characters hitherto suppressed, as will be referred to pres-

ently, but he will not admit the possibility of any gradual modification of a species by the response of the organism to external agencies.

Darwin he dismisses in a few words. "We go to Darwin for his incomparable collection of facts. We would fain emulate his scholarship, his width and power of exposition, but to us he speaks no more with philosophical authority. We read his scheme of evolution as we would that of Lucretius or of Lamarck, delighting in their simplicity and their courage." Bateson admits, as every one must, that natural selection has played a certain part in evolution, but he is very doubtful about its being more than a secondary factor. He is "even more skeptical as to the validity of that appeal to changes in the condition of life as direct causes of modification, upon which latterly at all events Darwin laid much emphasis." A belief held by Darwin and Huxley, and strenuously maintained by Herbert Spencer and Cope, and which no one has ever disproved, may, of course, be erroneous, but can hardly be dismissed thus lightly on the strength of experiments which have little or no direct bearing on the question.

Other views very generally held he brushes aside with equal confidence. "We have done," he says, "with the notion that Darwin came latterly to favor, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible." I do not know for whom Prof. Bateson speaks, but there are certainly still many who hold that modern research has abundantly proved the truth of Darwin's view that evolution has unquestionably been the result of the accumulation of small differences.

But let us look a little further at the suggestions Professor Bateson has to offer us in exchange for the old-fashioned views of Lamarck and Darwin. "This is no time for devising theories of evolution, and I propound none. But we have got to recognize that there has been an evolution, and that somehow or other the forms of life have arisen from fewer forms; we may as well see whether we are limited to the old view that evolutionary progress is from the simple to the complex, and whether after all it is conceivable that the progress was the other way about. I ask you, simply to open your minds to this possibility. It involves a certain effort."

Bateson considers that there is no evidence that changes ever take place by the addition of factors, but that there is satisfactory evidence that new forms have arisen by loss or fractionization of factors. "If then," he says, "we have to dispense, as seems likely, with any addition from without, we must begin seriously to consider whether the course of evolution can at all reasonably be represented as an unpacking of an original complex which contained within itself the whole range of diversity which living things present." As an example of this theory of unpacking he gives us the case of cultivated apples. "When the vast range of forms, size and flavor to be found among the cultivated apples is considered it seems difficult to suppose that all this variety is hidden in the wild crab-apple. I cannot positively assert that this is so, but I think all familiar with Mendelian analysis would agree with me that it is probable, and that the wild crab contains presumably inhibiting elements which the cultivated kinds have lost." The factors for the new forms have apparently been in the ancestors for countless generations, but kept down by other factors and only released when these others are by some agency removed. The fineness of merino wool, the multiplicity of the quills in the tail of the fantail pigeon, the scents of flowers and fruits are given as examples of releases of the factors which produce these results.

But still more startling is the statement with regard to the artistic faculty. "I have confidence," he says, "that the artistic gifts of mankind will prove to be due not to something added to the make-up of the ordinary man, but to the absence of factors which in the normal person inhibit the development of these gifts. They are almost beyond question to be looked upon as releases of powers normally suppressed." We have been told that no organism can hand on any factor which it did not itself receive in fertilization, from which it necessarily follows if no new factors can be added that the artistic factor must have been present in man's ances-

tors—the anthropoid ape, the labyrinthodont, and the fish. Perhaps it is the presence of this artistic factor that accounts for the marvelous beauty of the Radiolaria and many of the Foraminifera! The old belief in teleology which Prof. Bateson holds up to ridicule seems to me quite as worthy of credence as the view that the factor for the fineness of merino wool was present in the protozoan ancestor of the sheep.

While every one must welcome the brilliant and most important work being done by the Mendellians and cytologists, which has given us so much new light on the nature of heredity, we cannot admit they have helped us much to an understanding of the processes of evolution. They have shown us some reasons why each generation resembles the previous, but they have not thrown the faintest ray of light on the problem of why it is, though there is no manifest difference between two succeeding generations, that if we take the first and last of 10,000 or 100,000 generations, the differences are very appreciable. They even go the length, as Prof. Bateson does, of denying the fact, though the fact is beyond question.

Thanks to the brilliant paleontological work of Leish, Cope, Marsh, Osborn, and others, we have a very fair knowledge of the evolution of the horse, the camel, the rhinoceros, the titanothere, and of a number of other mammalian types. The experimenters discuss whether evolution took place by loss of factors, or by cross breeding, by slow changes or by rapid leaps: the paleontologist shows how it did take place and demonstrates that the evolution was gradual as held by Darwin, notwithstanding the remarks of Bateson.

When Marsh first called attention to the three or four most striking stages in the evolution of the horse, one might perhaps fairly have argued that the stages were too few to prove much; that there was no evidence that a *Mesohippus* had not more or less suddenly arisen from an *Eohippus*; and that there was no clear evidence of any gradual alteration. Now, however, all this has changed, and the difficulty is to define the limits of a genus like *Eohippus*, or of a species like *Mesohippus Bairdi*. The genera and species pass almost imperceptibly into others. The small low-crowned molar of the early *Hyrachtherium* has slowly and steadily through perhaps three million years evolved into the large complicated grinder of the modern horse. Are we to believe that this was because *Hyrachtherium* had in it the factor for producing a horse-like molar?

Cope has shown that the *Phenacodus*-like molar is the ancestral type from which all ungulate grinders are derived. Must we believe that the small *Phenacodus*-like form which was the common ancestor of all ungulates had not only the factor for producing horse-like molars, and the factors for all the intermediate stages, but at the same time the factors for producing molars such as are met with in the ox, the rhinoceros, the titanothere, the tapir, and the elephant? Had it also the factors for the antlers of the deer, for the trunk of the elephant, and for the loss of the hind limbs in the dugong, in addition to the factor for the fineness of merino wool?

The old views of Lamarck and Darwin may require slight amendment here and there, but they certainly have too much established truth to be altogether set aside. I quite agree that those zealous ultra-Darwinians who have endeavored to explain all evolution by the working of natural selection have done much to discredit the theory. But apart from the undue importance placed on natural selection by Darwin and his followers, there is no doubt that most of Darwin's work will stand the test of time.

Lamarck appears to have been the first scientist who clearly recognized the importance of the part played by the use and disuse of organs in the modification of animal types; and a large number of workers since his time have agreed that in function we have a prime factor. Darwin considered it played a secondary part, and there have been those who have argued that it played no part at all. Even now there are many who hold with Bateson, that the actions and habits of an animal cannot produce any changes which can be inherited by the offspring. They are willing to admit that the increased use of a limb will result in the increase of the muscles and of the strengthening of the bone in the individual, but they refuse to admit that the next generation will be influenced even in the slightest degree

*Science Progress.

by the action of the parent. I do not know what are Bateson's reasons for refusing to admit that acquired modifications can be inherited, but it has long seemed to me that the arguments of the opponents of the theory amount to this, that they cannot see how the sexual elements of an animal can be influenced by the habits of the animal, therefore they cannot be.

Those of us who hold that the actions of an animal do influence the next generation do not undertake to prove it experimentally. If it took 3,000,000 years or 1,000,000 generations to evolve the molar of the horse from the molar of *Hyrcotherium*, one need hardly expect to be able to demonstrate any perceptible change by experiments in a human life-time. Nor are we able to say how the offspring can be influenced. But we do say that the evidence is quite conclusive that it is influenced.

In the evolutionary series of the horse we see the gradual increase in size of the middle toe and the gradual dwindling of the side toes. It has been very plausibly argued that the middle toe has increased through nature favoring those forms in which it is better developed, and less plausibly argued that the side toes have dwindled and become lost through nature eliminating those types in which the side toes proved a slight handicap as against others in which the side toes were even more reduced. But if we consider all the change that has taken place in 1,000,000 generations it will readily be seen that at no time has nature ever anything very tangible to select. And as we have the clearest evidence that certain changes could not have been produced by natural selection we are probably justified in doubting if any have been.

It is well known that underground and cave-dwelling animals have usually small eyes or have entirely lost their eyesight. We cannot, of course, demonstrate an evolutionary series showing all the stages by which the eyesight has become lost in *Notoryctes* or *Chrysochloris*. But by examining other animals of somewhat similar habit we see various stages in the reduction of the eye such as may have been passed through. In *Georychus* the eye is small; in *Talpa* it is still more reduced. In *Chrysochloris* it is quite under the skin and pretty certainly functionless. In *Notoryctes* only a rudiment is left. The old natural selection arguments brought forward to account for the reduction of the eye of the mole are seen to be of no value when we consider the problem of the further reduction of the functionless rudiment of the eye deep below the skin.

In all the divisions of the vertebrates we have examples of increased development with increased function and reduced development with lessened function. But one of the most striking examples is the reduction in size of the wing in birds which have ceased to fly. It matters not whether the bird is a rail, a pigeon, or a goose, if it takes up its abode on an island where it is free from ground enemies it no longer requires to fly, and as a result of its gradual ceasing to use its powers of flight, the wing and its muscles gradually become reduced. Sometimes the reduced wing, though no longer useful for flying, may, by taking up other functions, be preserved, but if it does not the wing becomes more and more rudimentary. We know from the work of Jeffrey Parker that the struthious birds had flying ancestors. In the ostrich the wing, though no longer useful for flying, is still retained of fair size for other purposes, but in *Apteryx* and the moa it is quite rudimentary.

We see clearly the increased development of a part with use and the reduction and elimination with want of use, and we might at first readily assume that the modification is the direct result of the function, but there are good reasons to believe that this would not be quite a correct statement of the case, for even after an organ has ceased to have any function the rudiment still continues to decrease, and in the development of tooth cusps and many other structures we notice the increase taking place before the parts can be functional. We are therefore driven to believe that increase and decrease of parts are due to augmented or lessened stimulation.

I think we may safely conclude that evolution as we see it in the animal world, and most probably also in the vegetable kingdom, has been due to responses of the organism to changes in stimulation. The part played by natural selection has been the elimination of those types which have been unable sufficiently to respond.

I shall not in the present paper discuss how the organism responds to various stimuli, nor state what seems to me at least a plausible theory of how even slight changes in the parent may affect the germ cells, but of this I feel confident, that no theory of evolution by changes of stimulation, even though it requires the inheritance of acquired characters, will ever make such

demands on human credulity as the theory which suggests that all characters seen in all living organisms of to-day, including the artistic faculty and presumably poetic genius, were present as factors in the Protistan germ from which all have been descended.

Memorandum on Hardness*

By Sir Robert A. Hadfield, D.Sc., D. Met., F.R.S.

WHEN an inquiry is made as to what is meant by hardness, it is found this is a somewhat indefinite and vague term. When "hardness" is spoken of, no more definite statement is intended than if, for instance, the term "strength" is being discussed.

If anyone were asked to measure the strength of a certain specimen, he would immediately ask, "Under what conditions?" He would then be told that what was required was, for instance, what pressure or tension it would stand without extending (or being compressed) more than 10 per cent; or what load before it fractured?

Hardness is no more definite than this. Hardness, in other words, is not a specific property of a material. To make it specific it must be hedged round with definite instructions as to the limit to which the material must be deformed. A definite figure cannot be assigned for any property of a material unless this property is a specific one.

The writer's conception of hardness is simply "resistance to deformation." Now the resistance to deformation of any material depends on how much it is deformed—as a rule, the more it is deformed the greater is the load required (for some viscous materials, such as pitch, the deformation proceeds continuously under a constant small load). Further, the rate of increase of load with deformation varies in different materials.

It is useless, therefore, to fix an arbitrary amount of deformation and measure the load, for of two materials A and B, A may require less load than B to deform it 5 per cent, but B may require less load than A to deform it 50 per cent. Also, brittle materials will not deform at all.

The efforts to establish an academic idea of hardness are, therefore, limited to the resistance offered by a material free to flow, up to the point when it is just permanently deformed, which in the case of brittle materials, of course, means fractured. In elastic material this academic definition of hardness is nothing more than the "yield-point." To measure the hardness one has, therefore, only to measure the "yield-point."

This definite and specific property seems to be the real "fundamental hardness," and indeed the whole hardness of brittle materials.

According to this criterion manganese steel is of a soft nature. Its yield-point is low, a very small load producing permanent deformation. From this standpoint manganese steel, unless its character is altered by deformation, is really soft, yet in the ordinary acceptance of the term this material is considered very hard. Why is this? The explanation is that the ordinary term involves a loose conception of more or less (no definite amount) deformation; and the "hardness" is that of the more or less deformed material. Manganese steel is an extremely hard-wearing material, in spite of its natural softness, because the act of abrasion deforms the material locally, its resistance to further deformation increasing enormously thereby, and the material actually abraded off is not manganese steel in its natural state, but is the quite different material, deformed manganese steel. Manganese steel is soft—deformed manganese steel is hard. In a pulled tensile bar over 500 ball number has been obtained, and yet the material is just as non-magnetic as before, showing this physical change has taken place without interfering with its peculiar non-magnetic qualities. Deformed manganese steel has its own specific properties, quite distinct from manganese steel in its natural state or manganese steel subjected to other heat treatment than water toughening. Somewhat similar remarks apply to bronzes and other materials.

This is really the crux of the difficulty in trying to assign a value to the practical term loosely known as hardness. It is not sufficient, however, in all cases to measure the resistance of the material to deformation at the point of rupture. In the first place, it is not possible to deform any specimen of material so that at all points where fracture occurs the amount of deformation is uniform. There seems no way of measuring the resistance to deformation, i.e., the true "hardness," of deformed materials except in an approximate manner. In the second place, the action of deformation, in the practical use to which the material is

applied, is to deform some parts more than others, in no regular and definite manner.

In scratch tests a comparison is made between the breaking stress of two substances; as the pressure is being applied, stress is applied equally on the two materials, and they deform accordingly, until one of them reaches its limit of deformation, breaks off, and is then said to be "scratched" by the other. The other is "harder" by virtue of the fact that its breaking stress is greater, and the method places materials in a series of ascending "hardness," that is, ascending "breaking stress." Some idea of the relative "hardness" of materials, for example, in Moh's scale, might therefore be determined (only approximately as mentioned above—due to unequal strain and difficulties due to cleavage) by tensile or shear tests, measuring the breaking stress per unit area of the fracture.

Scratch tests, therefore, while probably enabling a definite comparison to be made of the true deformation hardness, do not give a numerical value.

Now all this applies only to ductile materials. Brittle materials which do not deform clearly cannot have this complication of deformation hardness. In these cases a practical measurement of hardness, which in reality is the stress at the yield-point, is possible.

There is here, however, a still further complication. Viscous materials, like pitch, while requiring in the ordinary way a definite stress to produce deformation and rupture, if sufficient time is allowed will deform under the slightest stress. The definition of natural hardness should, therefore, include an arbitrary time element, and this is a matter of practical arrangement. The time rate would theoretically be infinitely slow, but this would destroy the practical value of the definition, as it would make pitch almost infinitely soft, like a limpid fluid, while to all practical intents it is rather hard. The time rate of loading must, therefore, be within the ordinary limits of practical testing. It might be stated that many of the "elastic" materials behave somewhat like pitch.

It will be readily understood from the above what relation the results obtained from current methods of testing hardness have to the true hardness of materials. Each of these methods in the case of ductile materials deforms the material in its own particular fashion. It deforms one part of the material more than another, and the relative degrees of deformation are quite different according to the method employed. For example, in abrasion or scratch methods some portion of the material is completely detached, that is, deformed to the point of rupture; in indentation tests the material is generally not broken; in each case the remaining portions are deformed in varying degrees from nil to the maximum, and not in equal gradients. How, therefore, can it be expected that these methods should agree among themselves, or that the results should represent some specific property of the material? Further, with indentation tests, even with tests by the same method, on different materials the same defect exists. How, therefore, can any of these methods give a true numerical relation between the hardness of two materials, so as to say, for instance, that one material is twice as hard as another?

Summing up, the situation seems to be:

(a) Hardness as generally understood, is a loose term, not representing any specific property, but "resistance to deformation."

(b) The greater resistance in general of deformed materials to deformation (or deformation hardness) over that required to produce the first permanent deformation in the same undeformed material (or natural hardness) is involved to an indefinite extent in this term.

(c) No single value can therefore be assigned to the hardness of the material in this general sense. It can only be expressed by a complete stress-strain curve.

(d) The "natural hardness" can be measured, and is the "yield-point stress." Even this is subject to disability in the case of viscous materials like pitch.

(e) There seems to be no satisfactory method of determining "deformation hardness," that is, the resistance to deformation of material deformed practically to its fullest extent. Scratch methods enable comparisons to be made of this more or less definite property, but do not give a numerical value.

(f) Other methods professing to measure hardness measure the resistance of material deformed to varying amounts in different parts, and cannot therefore be expected either to agree among themselves or to give a true numerical value for the hardness.

(g) Brittle materials in which little or no deformation is possible are not subject to this complication of deformation hardness. On the other hand, they cannot be indented without fracture (cracking), and the utility of indentation methods is destroyed in their case.

*Report of the Hardness Tests Research Committee, read before the Institution of Mechanical Engineers. Reported in *Engineering*.

The Planetesimal Hypothesis*

The Result of the Close Approach of Two Suns

By Daniel Buchanan, M.A., Ph.D., Professor of Astronomy and Mathematics, Queen's University, Kingston, Canada

In a previous issue of the SCIENTIFIC AMERICAN SUPPLEMENT,¹ an attempt was made to show that the Nebular Hypothesis of Laplace, while accounting for all the motions of the members of the solar system known at the time the theory was formulated, could not be made to conform with the fundamental laws of dynamics or the facts revealed by subsequent astronomical discovery. In the present article a brief outline will be given of the Planetesimal or Spiral Nebula Hypothesis formulated about ten years ago by Professors Moulton and Chamberlain of the University of Chicago.

While the Planetesimal Hypothesis is mathematically sound and satisfies all the requirements of observational astronomy better than any other theory of evolution, it is not a complete genealogical tree which carries the bewildered reader through the roots, trunk and various ramifications of branches and twigs from the creation

natural course of astronomic life. It merely assumes that one of simplest and most inevitable of astronomic events, viz., the close approach of two suns, stimulated a partial disruption or deployment of one of these suns which, in turn, gave rise to our planetary system.

The solar system is but one of the many similar families which inhabit the sidereal universe. It is patriarchal and nomadic in type, obeying the undisputed control of the head and wandering leisurely through space with only an occasional encounter with similar families. The whole solar system, with its eight planets and their twenty-six moons, its eight hundred planetoids each revolving about the sun in periods ranging from 1.75 to 8 years, its zodiacal-light materials, its comets and meteors, occupies only a very small portion of the sidereal universe and is far removed from its nearest neighbor. Light, which travels at the rate of 186,000 miles per second, requires 4.5 hours to go from

thus discovered are called radial velocities or motions in the line of sight, that is to or from the earth. The phenomenon revealing radial velocities is similar to that which makes the whistle of a locomotive appear to have a higher pitch when it is approaching a station than when it is leaving. The apparent motions of the stars are not in general their real motions but are a compound of their real motions and the earth's motion. For instance, the stars in the neighborhood of the constellation Lyra appear to approach us with an average velocity of about ten miles per second. The stars on the opposite side of the heavens from Lyra appear to recede with the same average velocity. Not all the stars on one side approach or recede, but there is a great variety of velocity of approach and recession even on the same side. When the average of these velocities is taken the result stated is obtained. The conclusion is that these motions are mainly the apparent



Plate 1.—The Net-work Nebula in Cygnus.



Plate 3.—The Trifid Nebula in Sagittarius.

of matter up to "Trade Conditions after the War." It goes back only a generation or so in cosmic evolution and from the relative motions of the stars and the appearance of spiral nebulae now observed it constructs for the solar system an immediate ancestor, a spiral nebula similar to the thousands of spiral nebulae now known to exist. To be more explicit at the risk of being less accurate, it is like an attempt to explain to a schoolboy who had been orphaned when an infant what his parents were like, by referring him to the build, features and characteristics of the parents of some of his chums without taking him back even to the voyage of the Mayflower or the Norman Conquest.

The Planetesimal Hypothesis postulates no general destruction or re-creation of matter. It appeals to no event which may not properly be regarded as in the

the sun to the outermost planet Neptune, and Neptune is thirty times as far from the sun as we are. Inconceivable as is the magnitude of the solar system, it is relatively minute when compared with stellar distances. The sun's next door neighbor, a Centauri, is 4.5 light years distant, a light-year being the distance light would travel in a year. Some of the more remote neighbors are many times this distance from us, but perhaps definite numbers might better be omitted lest the writer's authority or veracity be brought into question.

The term "fixed stars" is a misnomer. "Fixed" stars are not fixed but are in motion. They have been called "fixed" because their motions are not discernible with the naked eye and to differentiate them from the so-called movable stars, that is the planets, whose motions may be observed during a relatively short period of time, a month or so. Stellar motions have been revealed mainly by the use of the spectograph. The motions

of the stars and that the sun itself with its retinue of attendants is sweeping through space with a velocity of ten miles per second. Although the velocity appears very great, the path lies in a region of "massive distances" and it will be nearly 600,000 before the sun draws near enough to Vega, its present apparent goal, to necessitate a campaign for "preparedness" or to break off diplomatic relations. The youth of our solar system was undoubtedly spent in a very different part of space from where it now is, but the computer assures us that 450 million years will yet be required for it to travel to the boundary of the stellar universe, the outskirts of the Milky Way.

These stellar motions lead to one of the simplest and most inevitable of astronomic events, viz., a "dynamic encounter" or a near approach of two stars or suns. As the average distance between stars is six, seven or eight light-years and as the average speed of the stars is thirteen miles per second, one star will visit the

**Queens Quarterly.*

¹"The Fallacy of the Nebular Hypothesis," vol. lxxxI, No. 2112, pp. 402, 403.

neighbor once in 80,000 years. When the lively character of the visit is considered we do not wonder that the stars prefer to spend their energies in traveling and not in "calling." Collisions between stars are exceedingly rare but a near approach may take place frequently.

Before considering the effect upon the stars of a near approach, let us turn our attention to some of the different kinds of nebulae which have been discovered. Astronomers are agreed that the earliest forms of inorganic life are the network nebula in Cygnus, Plate 1, the Orion Nebula, Plate 2, the Trifid Nebula, Plate 3, and the background of the nebulosity which embraces a great part of the Milky Way. There is no suggestion of order or system in these nebulae, but they appear as great seething clouds "without form and void." The spectrograph reveals that in many cases their substance consists entirely of glowing gases or vapors.

We must believe that they are endowed with gravitational power and are therefore in motion. By virtue of motions or of intruding materials which strike the nebula obliquely low rotations occur, more and more spherical forms are assumed, and the first stage of stellar life has arrived. But that is an earlier story.

With the introduction of photography into practical astronomy a great many nebulae have been discovered and the majority of these are spirals. The most beautiful spiral nebula is that in Andromeda, Plate 4. It appears to the naked eye as a hazy patch of light, but its spiral form is discerned only with optical aid. In 1898 and subsequent years the late James E. Keeler of the Lick Observatory made several photographs with the three-foot Crossley reflector and in the clear air of California mountain peaks and by long exposures, reaching up to five or more hours, he obtained several magnificent photographs of nebulae previously unknown. He estimated the number of spirals at 120,000, but the recent observations of Perrine with the same Crossley reflector and of Fath with the sixty-inch Mount Wilson reflector have shown that the number of spirals discoverable with fairly short exposures far exceeds Keeler's estimate.

It is a significant fact that the irregular nebulae such as Cygnus, Orion, the Trifid, etc., are in or near the Milky Way, while the spirals in the same region are negligible. The first group forms a part of our stellar system while the spirals are very far remote and of enormous dimensions. Their avoidance of the Milky Way, however, seems to show some close relationship with it.

The photographs of spirals in Plate 5 are typical of all spirals. They consist of a central nucleus from which extend two arms that wind about the nucleus like huge protective tentacles. The nebulous matter is not distributed uniformly over the arms but secondary nuclei or "knots" are collected at various places, while dark lanes mostly void of matter reach between the coils nearly up to the central nucleus. From the way in which the materials seem to be distributed in the nebula, it is apparent that its dimensions are not maintained by gaseous expansion as in the Laplacean theory, but by the separate motions of the various secondary nuclei.

Since the spiral is the prevailing form it is altogether improbable that several thousands of these nebulae should assume this very special shape as the result of mere chance. There must be some immediate cause and a satisfactory explanation can be found when we consider the disruptive forces generated on the near approach of two suns.

As two large bodies approach, the tendency to rupture through tidal strain increases as the distance between them decreases. In 1848 Roche showed mathematically that the tidal strain of a planet upon a fluid satellite of the same density as the planet would be sufficient to break up the satellite if the distance between them is less than 2.44 times the radius of the planet. The Roche limit for the moon is 11,000 miles. Thus, if the

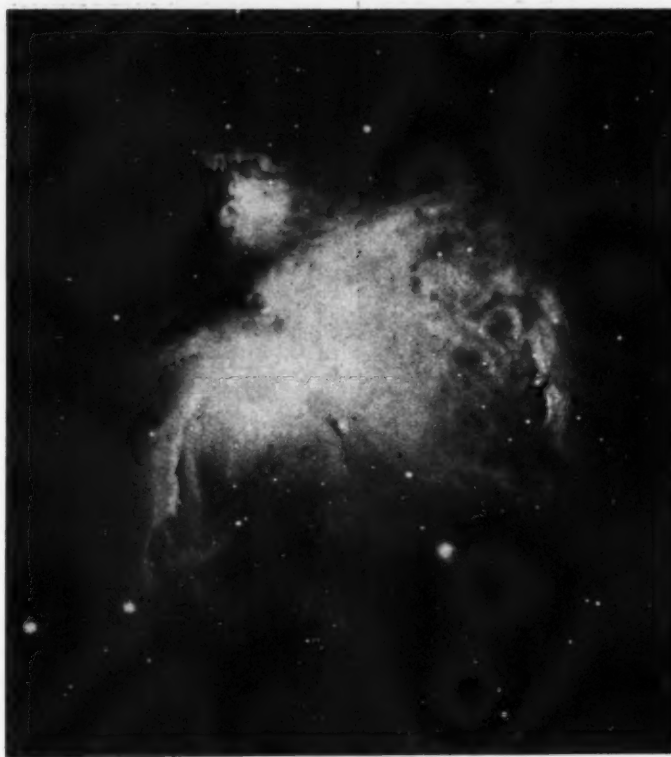


Plate 2.—The Great Nebula in Orion.



Plate 4.—The Spiral Nebula in Andromeda.

moon approached the earth to within this distance from the earth's center, tidal strains would be theoretically sufficient to shatter it into fragments the size of meteors or comets' heads. Besides this tidal strain there must be taken into consideration the eruptive tendencies of highly heated gaseous bodies. For example, vast eruptions, called *prominences*, are observed to rise up from the sun to altitudes ranging from 50,000 to 300,000 miles with velocities as great as 500 to 600 miles per second. If a gaseous star has been greatly compressed by its own gravity, its internal elastic stress

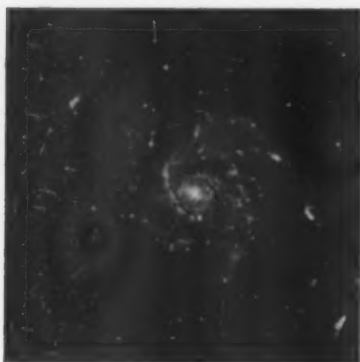
may greatly exceed its cohesion, and when a slight external gravitative control approaches, along the Kerry coast, for instance, the more or less enforced internal cohesion is momentarily relieved and Sinn Fein insurrections break out with characteristic violence.

Now consider the dynamic encounter or the near approach of the two suns S and S' , Fig. 1. We need consider only the influence upon S as a similar influence operates upon S' . The result will be the same whether the two suns approach on elliptical, parabolic or hyperbolic arcs. As the suns approach, the body S will become elongated toward and away from S' under the tidal strain. When they reach certain relative positions A and A' , the elongations become concentrated into tidal cones and eruptions a, a take place toward and away from S' . We might take an illustration entirely at random and picture the elongated star S as a gigantic Warspite passing swiftly through the lines of a hostile fleet (momentarily released from a secure harbor) and firing, fore and aft, gaseous bolts with a non-negligible solid nucleus, however, as it sweeps near a massive neighbor. It may be entirely unnecessary to add that the gaseous bolts in the illustration are not the result of mutual attraction as in the case of the suns. As S and S' move along their orbits the dispersive action constantly lies in the line joining their centers, while the position of the materials a, a is in the line of the resultant of the dispersive force and the direction of motion of S . Thus, when the suns reach the positions B, B' , the masses a, a will have moved from S along the dotted curved lines indicated, while new eruptions b, b take place along the line of readjusted attractions. Let us suppose that the two suns are nearest when at B, B' . Since the tidal strains upon S are then greatest, the bodies b, b will not only be the largest eruptions but will be hurled forth with the greatest velocities. As the suns proceed on their courses the materials a, a and b, b follow the dotted lines indicated, while new eruptions c, c take place. This process continues until the suns have separated sufficiently to prevent further eruptions. The amount of the star S which has not been deployed depends upon the shortest distance between the stars. If the approach is within the Roche limit of the more massive body, the smaller body may, theoretically, be entirely deployed, leaving little or no nucleus. If the approach be less near, the residual nucleus will be correspondingly greater. The probability is that the star S may not be completely disrupted but that a predominating nucleus may be left.

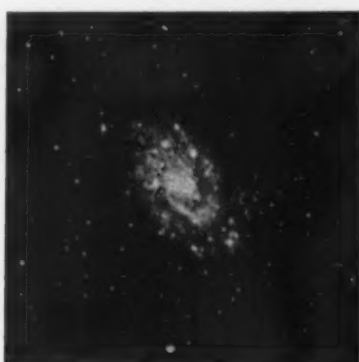
The two arms which so distinctly characterize the spirals are not the paths along which the ejected masses move but are the relative positions of these masses or "knots" as they move about the parent nucleus. Thus, in Fig. 2 the dotted lines represent the paths while the heavy lines denote the relative positions. The dotted lines, $1, 1'$ represent the orbits of the matter first ejected. Since S' was then at a considerable distance, that orbits would be smaller. The size of the orbits as well as of the ejected masses increases as the distance between S and S' diminishes. The particles describing the curves $3, 3'$ were ejected when S and S' were nearest, while the curves $4, 4', 5, 5', 6, 6'$ were described by particles cast off as the suns receded.

Some of the mass of S may be ejected with sufficient velocity to enable it to escape from the gravitative control of S and take new allegiance under S' . Other portions may reach neutral positions with respect to the great world war waged between the belligerent suns, but to be neutral in this conflict may mean being lost to the world in a literal sense as such portions may wander off to new suns entirely. The great majority of the disrupted material will remain under the gravitative control of the parent nucleus, some of it returning to the devastated territory in its original home while other portions maintain a separate existence.

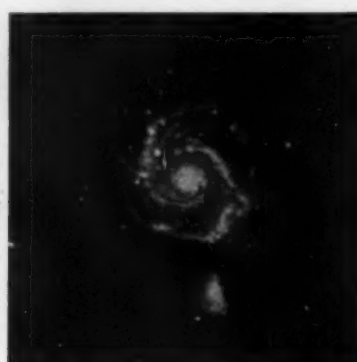
This then is the simple and reasonable explanation of



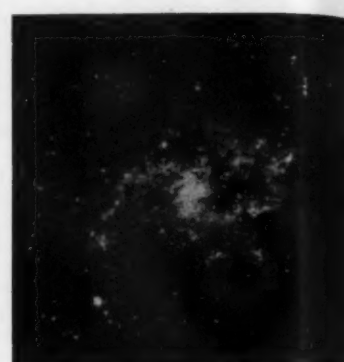
M 101, Ursa Majoris.



H. V. 44, Camelopardi.



M 51, Canum Venaticorum.



M 33, Trianguli.

the formation of a spiral nebula. Our solar system developed from such a nebula, and this development brings us to the second stage in the Planetesimal Hypothesis.

Our sun developed from the residual nucleus S , while the planets were formed out of the larger "knots" in the arms of the spiral. The planets were made to revolve about the sun, all in the same direction, by the attraction of the disturbing sun S' . As the "knots" moved in their orbits they would increase in size by the accretion of smaller masses which passed sufficiently near them. The larger masses were ejected when the disturbing sun was closest and they would consequently move away from S to greater distance than the materials first cast off. Thus we see why the major planets are at greater distances from the sun than are the minor planets. The outermost planet Neptune may have been the largest mass ejected, but the nucleus which developed into Jupiter gained more by accretion.

The planes of the orbits of the various planets are not coincident. They differ not only from each other

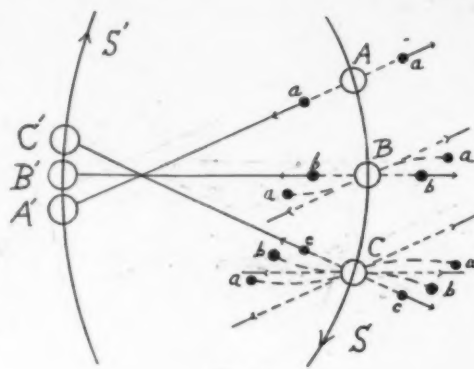


Fig. 1.

motion it is not necessary to postulate that the original sun S shall have a forward or direct rotation, that is

the direction in which the planets rotate is the result of impacts arising in the case of the sun from the ejected particle which had returned to it and in the case of the planets from encounters with scattered materials which crossed or approached the planetary orbits.

For the proof of this statement we must make an appeal to Celestial Mechanics, but if the reader would prefer to concede the fact rather than submit to its substantiation he may be spared the following mildly mathematical argument.

If a particle is subjected to a central force which varies according to the Newtonian law of the inverse square, the velocity with which it moves in its elliptical orbit having the center of force as a focus is given by the formula

$$V^2 = 2/r - 1/a,$$

where V denotes the velocity, r the distance of the particle from the focus, and a the semi-major axis of the ellipse. Suppose two bodies move in orbits C and C_1 about O , the center of force, and let us denote the semi-

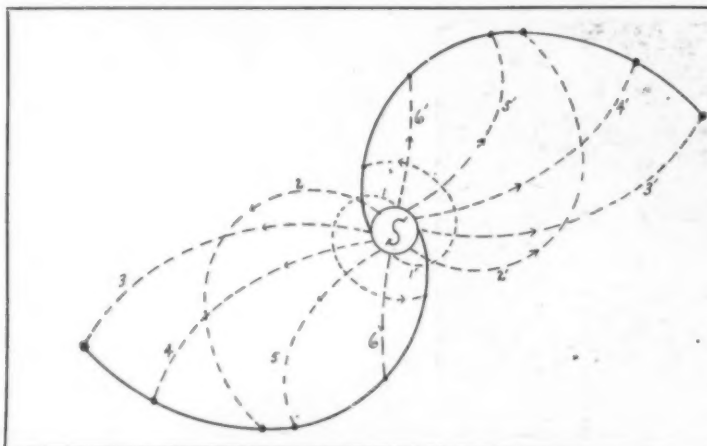


Fig. 2.

From Moulton's *Introduction to Astronomy*.

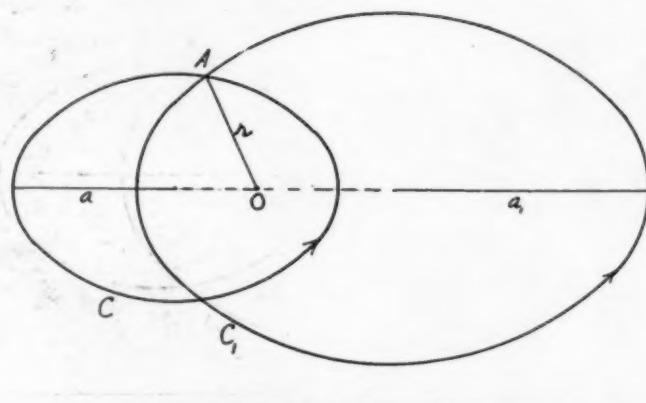


Fig. 3.

but also from the plane of the sun's equator. The inclinations to the mean plane of the system, however, diminish as the distances of the planets from the sun increase. These facts prove to be a very serious obstacle to the Laplacean theory but do not contradict the hypothesis under consideration. The nuclei which formed the inner planets were the first to leave S . As the disturbing sun S' was then at a considerable distance its attraction, while sufficient to produce an eruption, was not sufficient to bring the planes of revolution of the disrupted material entirely in coincidence with the plane of its motion. When the nuclei of the major planets were cast off the sun S' was sufficiently near to bring the planes of revolution more nearly to coincidence with the planes of their motion. Inclinations of all the planets would be diminished by gathering up scattered materials and, as the larger planets gained more through accretion, their inclinations to the general plane of the system would be correspondingly decreased.

How did the planets acquire their spheroidal shape and their rotation? The answer to the first part of the question is contained in the answer to the second as the oblate spheroid is a figure of equilibrium assumed by a rotating body. The shape of a planet is the result of its rotation. In order that the sun and planets shall have a rotary

in the direction in which the planets revolve and rotate, or indeed any rotation at all. Whatever may have been the direction of rotation of S , if it had any rotation,

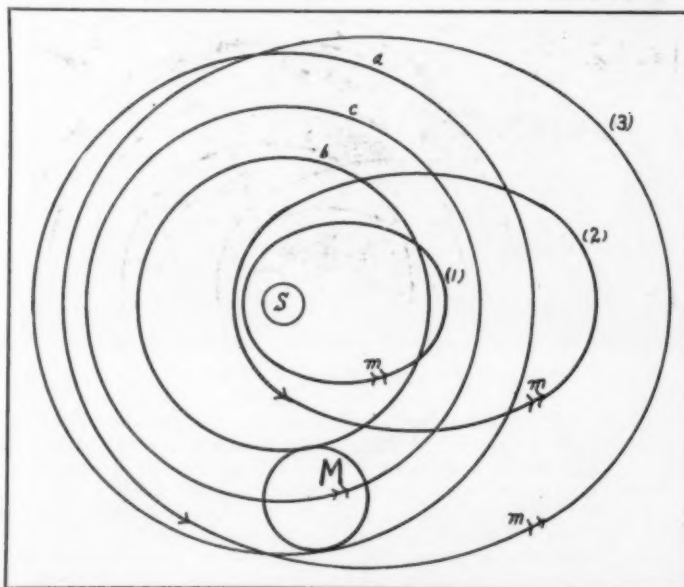


Fig. 4.

From Moulton's *Introduction to Astronomy*.

major axes by a and a_1 , respectively, Fig. 3. Now consider the relative velocities, V and V_1 , respectively, of the two bodies when they are at A the same distance r from the focus O . Since

$$V^2 = 2/r - 1/a \text{ and } V_1^2 = 2/r_1 - 1/a_1,$$

and since r is the same in both equations, it follows that V_1 is greater than V if a_1 is greater than a . Hence we have the lemma:

If two bodies revolve about the sun according to the law of gravitation, the body which moves in the orbit having the greater major axis has the greater velocity when both bodies are at the same distance from the sun.

Now let us apply this lemma to show that collisions with scattered materials tend to give the planets forward rotations. For simplicity in drawing we shall suppose in Fig. 4 that a planetary nucleus M revolves about the sun in a circle c . The argument would be precisely the same if the orbit were assumed to be an ellipse. Let us suppose that the nucleus M moves between the two circles a and b . There are three classes of orbits of scattered materials which M may encounter in its revolution, (1) those whose orbits lie within c , (2) those whose orbits cross c , and (3) those whose orbits lie wholly outside of c . Obviously only the orbits of (1) and (3) which cross the circles a and b respectively provide collisions. Consider first a collision

with a body m moving in orbit (1). Since the major axis of the orbit of M is greater than the major axis of the orbit of m , the planetary nucleus M has a greater velocity than m when both are at the same distance from the sun or when they collide. Consequently the planet overtakes the particle and since the collision takes place at a point on M between the planet and the sun, the resulting tendency is toward a *forward* rotation. When the planet collides with a particle moving in orbit (3) the velocity of the particle is greater than that of the planet, according to the lemma, and consequently the particle overtakes the planet. As the collision takes place at a point on the planet remote from the sun, the resulting tendency is toward a *backward* rotation. When the planet collides with a particle moving in orbit (2) the velocity of the particle is greater than that of the planet, according to the lemma, and consequently the particle overtakes the planet. As the collision takes place at a point on the planet remote from the sun, the resulting tendency is likewise toward a *backward* rotation. In both cases collisions may occur tangentially and these are more effective in producing rotation than those which occur centrally. In the case of particles moving in orbits of class (2), collisions may occur more or less centrally but these do not materially affect the rotation of the planet M . Tangential collisions tend to produce forward or retrograde rotations according as the semi-major axes of the orbits of the particles are less than or greater than the radius of the circle c . These opposing tendencies will about counterbalance and their influence on the rotation of the planet may be neglected. Hence the two classes of bodies (1) and (3) which are more efficient in producing rotation tend to give the planet a *forward* rotation while the tendencies of the less efficient bodies of class (2) mutually destroy one another.

According to the foregoing argument it would be expected that the larger planets would rotate faster than the smaller ones as they would receive more bombardment from the scattered materials and at points farther from the axes of rotation. The following table speaks for itself:

| Planet. | Equat. diam. in miles. | Period of rotation. |
|---------------|---------------------------|--|
| Mercury | 2,765 | 88 days, rotates once in a revolution |
| Venus | 7,826 | 225 days, rotates once in a revolution |
| Earth | 7,918 | 24h |
| Mars | 4,352 | 24h 37m |
| Jupiter | 86,881 | 9h 50m to 57m |
| Saturn | 76,470 | 10h 38m |
| Uranus | 34,900 | 10h to 12h |
| Neptune | 32,900 | unknown |

The effect of tidal friction of the sun upon Mercury and Venus has been sufficient to alter whatever periods of rotations they may have had so that the planets now keep approximately the same face toward the sun. This condition of affairs exists between the earth and moon. The sun exercises a similar influence over the other planets, but it is inappreciable, owing to their greater distances.

The forward rotation of the sun itself in a period of about twenty-five days can likewise be accounted for without attributing a similar rotation to the original nucleus. The enormous tides produced on S would tend to give it a rotation in the direction in which S' moved. Further, a considerable quantity of the material which was ejected and had its straight line orbit changed to an elliptical orbit would again return to the sun and in such a way as to give the sun a forward rotation. Both of these influences were more predominant in the equatorial region and that accounts for the observed phenomenon of equatorial accelerations, that is, the equatorial regions rotate faster than the higher latitudes.

Let us now consider the origin and revolution of the various satellites. When the planetary nuclei left the sun they were accompanied by smaller secondary nuclei which had sufficient velocities to prevent them from being precipitated upon the planets or the sun. These independent nuclei formed the satellites.

When such a body assumed the new rôle of satellite, it could revolve about its primary in either the forward or retrograde directions and in a plane which might be highly inclined to the plane of the planet's orbit. Consider first the case of satellites with highly inclined orbits. High inclinations, as in the case of the satellites of Uranus and Neptune, would not be expected on the basis of the Planetesimal Hypothesis, but they do not directly contradict it. Whenever such a satellite passed through the plane of motion of its primary, that is twice in a revolution, it would encounter scattered materials and this would tend to decrease its speed of revolution. With a retardation in revolution and an increase in the mass of either primary or satellite there would be a corresponding decrease in the size of the satellite's orbit. If the satellite were sufficiently

far away from its primary when it was "captured" so as to endure these successive diminutions in orbit until all the obstructing materials had been cleared away, its separate existence would be assured; if not, it would be precipitated upon the planet. Thus only a few satellites having highly inclined orbits survived the strenuous days of infancy and now triumphantly present themselves as almost insurmountable obstacles to any theory of cosmogony.

Satellites having low inclinations to the planes of their respective primaries move in both the forward and retrograde directions but forward revolutions predominate. The retrograde satellites are the eighth and ninth of Jupiter and the ninth of Saturn. If we consider in Fig. 4 the planet to be situated at the center of the small circle M and the satellite to move on the circumference of this small circle, then the arguments used to show that collisions tend to give a forward rotation to a planet may be used here to show that similar collisions of a satellite with the scattered materials which it encounters tend to increase or decrease its velocity of revolution about its primary according as the motion is direct or retrograde respectively. When there is an increase or decrease in this velocity there is a proportionate increase or decrease in the size of the orbit. On the whole, collisions tend to prevent a direct satellite from being drawn upon its planet as both primary and satellite increase in size. There is just the opposite tendency in the case of a retrograde satellite. Hence the chances for a retrograde satellite to preserve its separate existence are very few unless it was far removed from its primary where there would be less frequent collisions and a greater reserve of orbit to sacrifice before the satellite would capitulate. Retrograde satellites are consequently fewer in number than direct satellites. When a planet has both kinds of satellites we would expect the retrograde to be more distant than the direct and this is actually the case with the retrograde satellites which have been discovered.

Three phenomena which contradict the Nebular theory remain to be discussed. They are the revolution of Mars' satellite Phobos, Saturn's rings and the planetoids.

The difficulty with Phobos is that it persists in revolving about three times as fast as the planet rotates. The period of the satellite was originally longer but with the increase in size of both itself and the planet the period decreased until it assumed the present rate. The rings of Saturn are within the Roche limit of the planet and the disruptive tendencies of the tidal strains of the planet more than counterbalance the accumulating tendencies of any predominating nuclei. Collisions have been frequent in the rings, but they have tended to pulverize the nuclei. Divergencies of motion have been destroyed and all the particles move in the same plane but with varying periods. The planetoids have remained as separate nuclei because the collective tendencies of mutual gravitation have been overcome by the disturbing influences of the nearby massive neighbor Jupiter.

Having thus brought the solar family into its primitive state of existence the Planetesimal Hypothesis leaves to the geologist the comparatively simple task of clothing a planet with an atmosphere and making it habitable where possible or capable of showing "canals" to a neighbor.

In the spring evenings the sunset sky is illumined with a soft hazy wedge of light stretching up from the horizon—the zodiacal light. The mathematical astronomer who adheres to the Planetesimal Hypothesis looks upon this wedge of light with mingled feelings of delight and dread; delight, because he believes that this light is produced by the small particles which must hover near the sun according to his theory; dread, because he knows that these particles may contain the primitive but pulverized remains of a planetary system which eked out its merry round of time prior to the aspirations of S' for "a place in the sun" or for "world dominion." When our sun has again passed near another unfriendly neighbor, another world war will ensue. Sacred treaties in the form of Nautical Almanacs and Ephemerides will become obsolete "scraps of paper." Old kingdoms will shoot forth as gas-bolts more or less poisonous according to geographical location, planets and satellites will pour forth as flaming liquids, new nuclei will spring forth exultantly and more glorious dominions will arise and be peopled in due course by supermen of superior culture. In their spring sky another soft hazy wedge of monumental light may once more be visible at sunset, but let us hope it will be a warning to beware the world menace enunciated in the Planetesimal Hypothesis.

Perpetual Plates for Photography

THE recent rise in price of photographic materials lends interest to a method newly devised by French

photographers for making use of the same plate to secure a practically unlimited series of negatives. The method is not adapted to all sorts of photographs, since considerable length of time is required for the exposure, and because of a lack of delicacy of detail, but for suitable subjects it not only works well, but naturally effects large savings. As outlined in *La Nature* (Paris) it is as follows:

In 1839 Daguerre noticed that a plate coated with calcium sulphide, exposed in the dark room and afterward placed upon a surface prepared with iodide of silver, made an impression on the latter of such nature that when it was submitted to the action of mercurial vapors a faithful reproduction was obtained of the image delineated by the objective. Shortly after Edmond Becquerel proved that red, orange, or yellow rays destroy phosphorescence. In 1880 these phenomena were applied by Darwin to the production of counter-types. A plate having a phosphorescent surface was first exposed to the sun for several seconds, then placed in contact with the negative to be reproduced and exposed to the light under a red glass for one or two minutes. The red rays extinguished the phosphorescence in proportion to the transparencies of the phototype; so successful was this action that it sufficed to place the phosphorescent plate against a gelatino-bromide plate, in the dark, to obtain upon development a positive print of the initial negative.

More recently M. Georges Bellais has perfected this method, rendering it applicable to the reproduction of images in the dark room and to prints on paper. A plate with phosphorescent surface is exposed to the full light; it is then placed in the camera as if it were a gelatino-bromide plate, and exposed in the apparatus, to the focus of an objective provided with a yellow or orange screen. A phosphorescent negative is thus secured, which, placed in contact with a gelatino-bromide paper, yields a positive print. This print is obtained as usual by developing and fixing.

As for the phosphorescent plate, it is sufficient to expose it afresh to the light to efface the image; it thus becomes capable of receiving a new impression in the camera, and of furnishing, in consequence, other photo-copies. The obvious advantage of this is that all the negatives are produced on a single plate, without developing, fixing or washing.

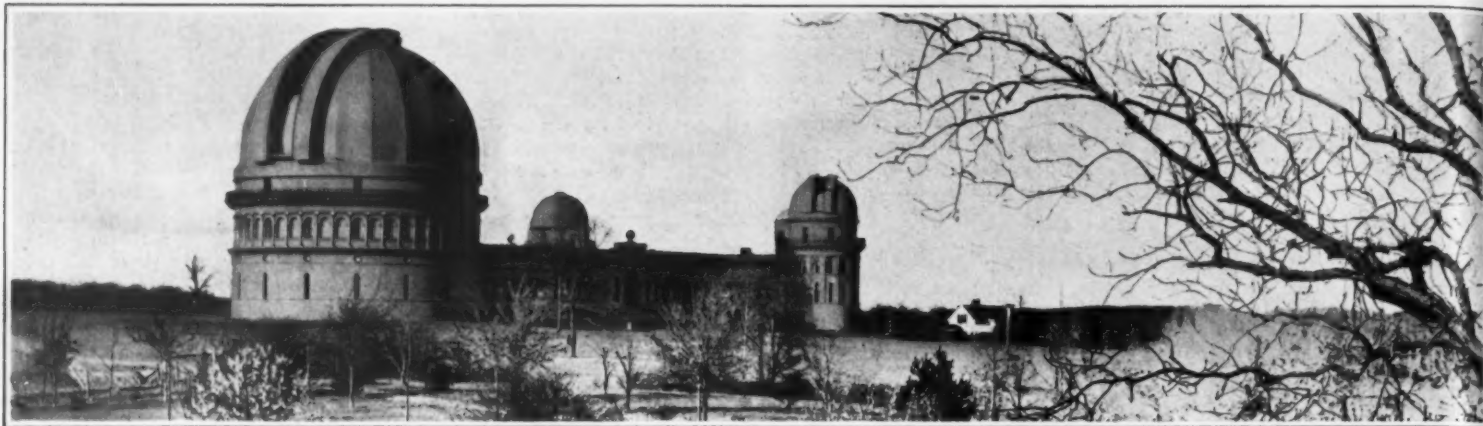
Let us add at once that this very seductive method is unhappily limited to a very small number of cases. In the first place, it is not suited to animated subjects or to poorly lighted views. The length of exposure demanded forbids its application except to reproductions or very well lighted landscapes. (However, under-exposure can be rectified by prolonging the contact between the phosphorescent surface and the sensitive paper for several hours or even entire days.) In the second place, the images lack delicacy and have a granular aspect, which is not suitable for all subjects. This defect inheres in the very constitution of the plate, which is prepared by sprinkling with calcium sulphide of a violet phosphorescence a sheet of cardboard, of glass, or of slate, previously coated with a pitchy varnish.

If, in order to reduce the granular effect, the sulphide be ground to an impalpable powder, one risks altering it so as more or less to destroy its phosphorescent properties.

As to the colored glass to place over the objective, M. Bellais considers the Lumière screen for autochromes to be the best. . . . The economy is obvious, since all the proofs are printed while sheltered from actinic light under the same plate, which serves successively as a negative for all the subjects to be reproduced.

The Expansive Power of Lime

THE expansion of quicklime when wet develops an enormous force that acts slowly and almost irresistibly, and has long invited use for mechanical purposes. Successful efforts to make use of this force have been noted in a recent issue of *Rock Products*, that describes its efficient use in breaking up heavy brick masonry. A number of 12 feet by 20 feet piers, 12 feet high, was situated between similar foundation piers for engines in operation, and it was necessary to remove them without injuring the machinery. Blasting was therefore inadmissible, and hand cutting and breaking too slow and expensive. The work was accomplished by drilling 3-inch vertical holes 3 feet deep and 3 feet apart in both directions over the entire areas of the piers and filling them within six inches of the top with fresh slaked lime in pieces $\frac{1}{2}$ inch to $1\frac{1}{2}$ inches wide. As soon as the lime was thoroughly wet the tops of the holes were filled with brick drilling well tamped, and in about ten minutes cracks started in every direction and the entire foundation pier was broken into cubes.



The Yerkes Observatory, at Williams Bay, Wis., in connection with the University of Chicago.

Two Important American Observatories

Where Practical and Theoretical Work of Great Value is Done

AMERICA is excellently equipped for astronomical investigations, for it possesses a number of splendid observatories which are equipped with some of the largest and finest instruments in the world; and the men in charge of these institutions, past and present, include names that occupy the highest positions in the scientific world. Most of these observatories are engaged almost exclusively in research of a theoretical nature, but in some the practical side of astronomy occupies the attention of the staff. Of the latter class the United States Naval Observatory, at Washington, is the best known, for it is this institution that produces the American Ephemeris and Nautical Almanac, and the American Nautical Almanac, which are indispensable to navigators, both in the Navy and in mercantile service. It also supplies the United States east of the Rocky Mountains with the correct time, and supervises the navigating instruments used both in the Navy and the Naval Air Service, and in performing these duties continuous fundamental observations of the heavenly bodies are kept up. Much other work of theoretical scientific value has also come from the institution.

As the Naval Observatory is so closely connected with the everyday interests of so many people, something of its history may be desirable. Under the authority of the Navy Commission, granted December 6, 1830, Lieut. L. M. Goldsborough established in that year a bureau for the care of the charts and instruments of the Navy, and at this bureau Lieut. Goldsborough, and other officers connected with the depot of charts and instruments, regularly rated all chronometers belonging to the Navy for some months by the aid of sextant and circle observations, and later with a thirty-inch transit instrument. This instrument was mounted in a small circular building on Capitol Hill, on a pier extending twenty feet below the level of the surface. This was the first astronomical instrument used by the Navy in Washington.

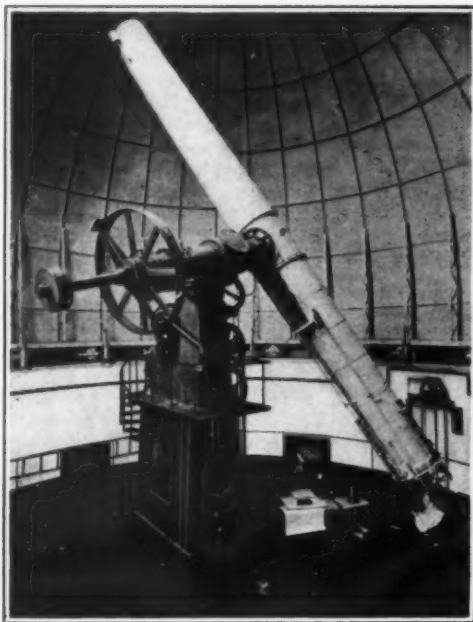
In 1853 Lieut. Wilkes, who succeeded Lieut. Goldsborough, erected a small observatory, sixteen feet square, about 1,000 feet north from the dome of the Capitol. In this was mounted a five-foot Troughton transit, made for the Coast Survey in 1815. No systematic observations were made here, however, except those for the determination of time.

During the absence of Lieut. Wilkes on the United States Exploring Expedition (1838-42), Lieut. Gillies made a series of observations of Moon culminating states, occultations and eclipses for determining differences of longitude between the depot and the stations occupied by the expedition; and with additions to the instrumental equipment recorded a large number of transits of the Moon, planets, and about 1,100 stars, and many other observations. He recommended that provision be made for magnetic and meteorological observations, which received the approval of the Secretary of the Navy, and a small building was erected for the purpose. It was directly due to Lieut. Gillies' influence that a new observatory of a permanent character was established in 1842, when Congress authorized a building for the housing of charts and instruments, not to cost over \$25,000.

The new building was erected in the southwest part of the city, and the instrumental equipment selected comprised a 9.6-inch equatorial telescope, a meridian

transit of 5½-inch aperture, a prime vertical transit of 4.9-inch aperture, a mural circle with a 4.1-inch telescope and a circle of 5 feet diameter and a 4-inch comet seeker. The collection of magnetic instruments consisted of a declinometer, a bifilar magnetometer, a balance magnetometer and a Fox deflector. The meteorological instruments included a standard barometer, dry and wet bulb self-registering thermometers, hygrometer, anemometers and a rain gage. Seven hundred volumes of standard works were purchased for the library, and numerous gifts of books were received from societies and observatories in Europe.

This was the first real Government observatory, and it was ready for use at the end of September, 1844; but through political and official mismanagement the work of the following years up to 1861 was of little value. In that year Capt. Gillies, the founder of the



The twenty-six-inch equatorial telescope in the United States Naval Observatory, at Washington, D. C.
Focal length, 289.66 inches.

institution, was appointed to the superintendency, when the original plans for making it a national observatory were resumed.

In 1865 an 8.52-inch meridian circle was mounted at the observatory, and in 1873 a 26-inch refracting telescope, by Alvan Clark & Sons, was added to the equipment. This was the first important instrument of American make placed in the observatory, all the others being of foreign manufacture.

In 1880 a new building was provided for by Congress, but the building on Georgetown Heights was not occupied until 1893. In this establishment the instruments installed were for the most part new, or the old instruments were greatly remodeled. The 9.6-inch telescope was replaced by a new refractor of 12 inches. A

meridian circle of 6-inch aperture, and a 5-inch alt-azimuth were added. The 8.5-inch instrument was given a new objective of 9.1 inches and the mounting reconstructed, as was also that of the 26-inch equatorial.

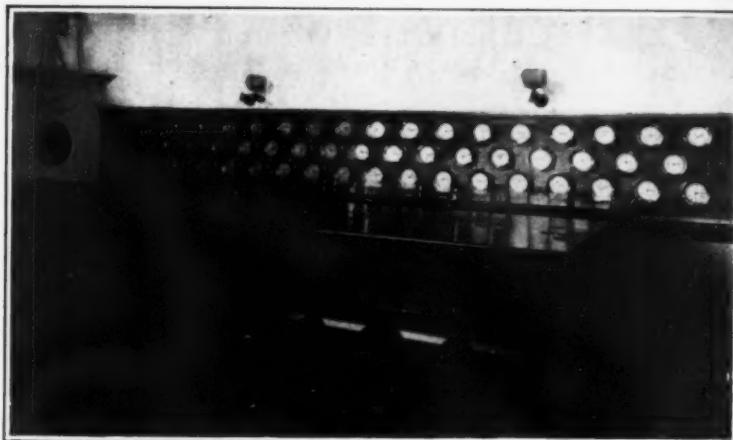
A few details relating to the 26-inch instrument may be of general interest. The tube of the telescope is 372 inches long, and has a diameter of 2 feet 8 inches at the center, 2 feet 5 inches at the object end and 2 feet at the eye end. The central section of this tube is of cast steel, for taking the attachment to the axes, and the other sections are of rolled sheet steel. The foundation is a solid concrete column, 18 by 21 feet in section and 16 feet in height, sunk 12 feet below the basement floor. Upon this pier is placed a cast iron column 16 feet 10 inches in height, which carries a head consisting of a substantial casting that carries the axes of the instrument. The clear aperture is 26 inches, and the principal focal length is 389.66 inches. The crown glass is 1.884 inches thick at the center, and weighs 70 pounds; the flint glass is 0.958 inch thick at the center, and weighs 110 pounds. The polar and declination axes are of hard, forged steel, the former 8 feet 6 inches long and 9 inches in diameter at the upper end, tapering to 8 inches at the lower end, and is bored out 2½ inches through the center. The declination axis is approximately 7 feet 6 inches in length, with a diameter of 8 inches at the upper end, and tapers to 7 inches at the tube. It is bored out like the polar axis. The dome that shelters the instrument is of steel, 45 feet in diameter, and having a slit 6 feet 2 inches wide running from the base to a point a few feet beyond the zenith. An elevating floor to accommodate the position of the observers to that of the instrument is provided, and has a rise of 12 feet.

Although the United States Naval Observatory is essentially an institution for practical purposes, a great deal of theoretical work of considerable importance has emanated from it, for its staff has included the names of many men of high standing in the world of science. The list is too long to be reproduced in these notes, but mention may be made of Profs. Simon Newcomb, Asaph Hall and William Harkness, although there are many others who have made their mark.

An observatory of a different class, it being devoted to scientific investigation exclusively, is the Yerkes Observatory, at Williams Bay, Wis., in connection with the University of Chicago. This noted institution had its inception in the private observatory of Prof. George E. Hale, who is now director of the Mount Wilson Solar Observatory, in California. Two very perfect glass disks (crown and flint), 42 inches in diameter, sufficient for an object glass of 40 inches clear aperture, had been cast by a well known maker of optical instruments in Paris for a California institution that found it impossible to raise the money necessary for figuring and mounting the telescope. President Harper, of the University of Chicago, and Dr. Hale approached Mr. Charles T. Yerkes with a proposition to purchase these disks in the interest of the university, and he agreed to finance the undertaking, and thus the largest refracting telescope in the world was assured to the university. These disks were purchased and placed in the hands of Alvan Clark & Sons to figure, while a contract was made with Warner & Swasey, of Cleveland, for the construction of a suitable mounting. This was in 1892, and plans for the new institution were immediately undertaken,



Transmitting clocks, chronographs and switchboards used in sending out the time to the country from the United States Naval Observatory.



Testing deck clocks for United States Naval vessels in the Time Service Room of the Naval Observatory, Washington, D. C.

with Dr. Hale as the director. Owing to the atmospheric conditions in Chicago it was not desirable to erect the new observatory there, and it was finally decided to locate it at Williams Bay, on Lake Geneva, in Wisconsin. Here a substantial building 326 feet in length was erected, which provided for the numerous offices, laboratories, workrooms and shops required, together with accommodations for the big telescope, and numerous other instruments.

To afford room for the big telescope a large dome 90 feet in diameter was designed, having an opening 11 feet wide. The telescope, which is mounted 62 feet above the ground, is 62 feet long, and the spectroscopic attachments used with it add ten feet to this length. The tube itself weighs six tons, and with all the moving parts added the total weight is about twenty tons; but so carefully balanced is the instrument that it can easily be moved by hand, although this work is ordinarily performed by electric motors. The rising floor fitted in the dome is 75 feet in diameter, and has a range of operation of 23 feet. Some of the attachments of the big instrument include a filar micrometer, which enables the observer to determine very exactly the angular separation of two objects, both of which are visible in the eyepiece at the same time, and to fix the angle made by the line joining the points with the north-and-south direction. Another attachment enables the telescope to be used as a camera, and with it some of the best photographs of the Moon, star clusters and some nebulae thus far secured have been made. A stellar photometer, by which the brightness of the stars is measured, is another attachment much used. Much attention is given to stellar spectroscopy; and in the study of the Sun an ingenious spectrohellograph, invented by Dr. Hale, has been found of great value.

Other apparatus in the equipment of this institution include a 2-foot reflector, which, for some purposes, is quite as powerful as the 40-inch telescope itself, and which, with its mounting, was made in the shops of the observatory. One of the smaller domes also contains the Kenwood 12-inch equatorial refractor, which is equipped with several useful attachments. A Brashier comet seeker is mounted in a small house on the roof of the building. The equipment for photographic work is quite complete; and the outfit of the institution includes a variety of other instruments and apparatus essential to the character of the work prosecuted here.

The results of investigations at the observatory are published in various American and foreign journals, those of an astrophysical character generally appearing in the *Astrophysical Journal*, of which the director of the observatory is the editor. Undergraduate instruction is not given at the observatory, but is provided for at the University of Chicago. Graduate students, however, who are competent in observational work in astronomy and astrophysics, are welcome, and may become fellows under university regulations. Astronomers from other institutions frequently take part in the regular work of the observatory; but the pressure for time for their scientific use makes it impossible to permit general visitors to look through the various instruments, although opportunities are given for inspecting the institution. The director of the Yerkes

Observatory is Prof. Edwin B. Frost, with whom is associated Profs. S. W. Burnham, Edward E. Barnard and an able corps of assistants.

Chemical Nuts to Crack

DR. RAYMOND F. BACON, the director of the Mellon Institute of Industrial Research of Pittsburgh University, presented a paper before the New York Section of the Society of Chemical Industry at Rumford Hall in the Chemists' Club at a recent meeting.

The following are a few of the problems he mentioned as in urgent need of solution.

Brown iron ore of the South. The problem here is to concentrate these brown ores of Virginia, Tennessee, Kentucky, Georgia, Alabama and Texas so that the iron content is brought up to fifty per cent. In this condition they would be readily marketable. Other iron ore problems that are waiting to be solved are

ducing it for about twenty-five cents a pound is discovered it will find a great use in alloys and for other purposes. It is lighter and stronger than aluminum.

Knowledge is wanted as to the chemical nature of coal as it comes to us. Method for making smokeless briquettes from anthracite culm is needed and so is a use for waste from bituminous coal washers.

New uses are wanted for gypsum, slate and for natural emery and corundum, which are being replaced by artificial abrasives. An exhaustive study of fuller's earth is needed; we do not know enough about it.

One of the most urgent needs is better refractory materials; that is, fire brick, furnace lining, etc., that will keep their shape at very high temperatures and that will not flint with any acids or alkalis that may turn up in the material they are designed to contain. Many problems could be solved if we had this.

In the coal tar industry new uses are wanted for those bodies that now go chiefly into munitions. If larger uses in the arts of peace are found for them, the hazard of attack in war will be decreased because of the availability of raw materials for munitions.

Greatly needed is a means of treating coal gas pitch so that it may be used in the place of natural asphalts for roads and streets. Still further investigation is needed in the problem of producing gasoline from coal gas. It is under more lively consideration in England and other countries which produce no petroleum.

Better methods are needed for producing petroleum bodies from shale. At present there is far too much waste, and there are such deposits of oil shale in America that the problem is of more than ordinary interest.

Many plants are wasted for lack of knowledge. Better methods are needed for extracting rubber from guayule sap, wax from candelilla and dyestuffs from Osage orange and algerita. What are the commercial possibilities of bear grass? More study is needed on the subject of making paper from rice straw and cotton stalks. On the other hand, the castor bean, sunflower and camphor trees are well adapted to the coastal plain of Texas and should form the bases

of well established industries. Some problems in cotton are the real action of fertilizers on yield and quality, a chemical treatment of the boll weevil and the prevention and bleaching of stained cotton.

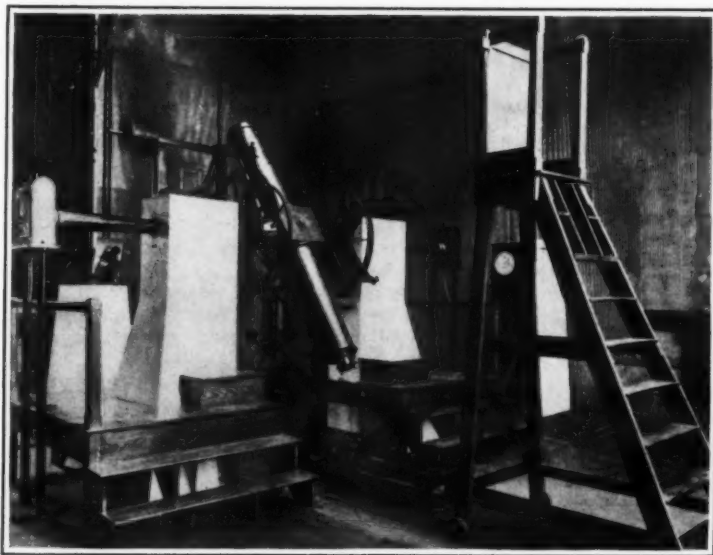
In the turpentine industry, despite great advances, there is still a loss of nearly one third of the volatile oil and better methods are needed to bleach resin.

The need of raw materials to make paper cries aloud in the soaring prices. Dr. Bacon suggests more research in the use of wood fiber from turpentine production and the manufacture of tannin from palmetto roots and paper pulp from the fiber residues.

These are only some of the things he called for. There's plenty of work for the chemist to do.

A Discovery of Prehistoric Ice

A SWEDISH expedition to Spitzbergen has made several excavations in the old moraines on the shore, resulting in the discovery of deep-seated ice, many thousands of years old. How thick a covering of rock such fossil ice can support is a question worth solving. At any rate, deep borings have proved that it does not continue under the floor of the harbor.



Nine-inch transit circle in the United States Naval Observatory as remounted in 1892.

the economic use of Tennessee clay ores and some potash-bearing (gray) ores of other Southern States.

In blast-furnace practice information is wanted about how much sulphur can be expelled with the top gases, and more knowledge is asked for on the effect of variations of carbon in iron. How can manganese be recovered from slags that contain a lot of it?

More information is wanted on the use of open hearth and converter steel slags in the pottery industry. How can the grade of pig iron made with coke be brought up to that made with charcoal? How can by-products be recovered from blast furnace gases?

Thousands of tons of arsenic and hundreds of thousands of tons of sulphur may be separated from the gases of copper and lead smelting works. Uses for this vast quantity of these products are wanted.

In the brass industry great quantities of zinc are lost in the fumes. A method of recovering this is needed.

Calcium, which is the metal of which lime is the ore, could be produced economically by methods now known, if uses for it could be found. With metallic magnesium, on the other hand, if a method of pro-

Treatment of Overexposed Photographic Negatives*

The Prevention of Reversal or Solarization

By R. E. Crowther

Of the various conditions which lead to the formation of a reversed result when an exposed photographic emulsion is developed in the ordinary way, this communication deals only with those which may be conveniently grouped under the term "Over Exposure."

Such over exposure may be constituted by abnormally prolonged action of a light of average intensity or by relatively shorter exposure to a highly actinic light.

The former condition is frequently encountered in the photographing of interiors when the subject includes unscreened windows at one end of the actinic scale and heavy shadows at the other end of the scale; the latter condition may be met with in the photographic investigation of various light sources such as the sun, the electric arc, etc. Jannsen (*Comptes rend.*, 90, 1447; 91, 119) observed, when photographing the sun, that as the exposures were prolonged the developed results were positives where they should have been negatives, and also that still longer exposures yielded negative developed results again.

These phenomena of reversal and re-reversal have excited considerable interest among photographic experimenters, their cause has been the subject of much speculation, and means for their prevention have been sought by many investigators with varying degrees of success.

The so-called gelatin dry plate process originated in 1871 in some experiments of R. L. Maddox, who prepared a gelatin emulsion which, while being very imperfect, was nevertheless workable. Maddox's emulsion contained an excess of silver nitrate and was developed with a plain solution of pyrogallol. In 1873 it was pointed out that removal of the excess silver nitrate and of the soluble reaction products led to improved results, i. e., in the presence of gelatin any other halogen accommodator was not necessary, and that bulk washing of the prepared emulsion could advantageously replace the tedious process of dialysis. The advantage of precipitating the sensitive compounds in the presence of a little gelatin was also first commented on in this year.

In 1878 Charles Bennett discovered the increase in "speed" which resulted from the preparation of the emulsion at the temperature of boiling water, at which temperature it should be maintained for some time before washing out the soluble reaction products. The use of a little ammonia in addition to heating, or even without heating, the emulsion for the production of greater speed, is perhaps the only published modification of Bennett's process which is of interest, and, although the details of the manufacturing processes of the modern dry plate are well guarded trade secrets, it may be taken for granted that, as far as essential ingredients are concerned, Bennett's formula is typical. The gelatin in the emulsion of the modern dry plate therefore acts both as the emulsifier and the sensitizer. While its properties as an emulsifier leave little to be desired, as a sensitizer it is far from ideal.

The ideal sensitizer would exhibit the following properties: Absolute stability under temperate, tropical or frigid atmospheric conditions; power to accommodate the halogens instantaneously, yielding stable compounds having no action on emulsion, developer or image. Solubility in water and alcohol and a colloidal structure are also desirable characteristics. Such a sensitizer has yet to be discovered.

Various substances are at present employed as sensitizers, e. g., a soluble silver salt is used in printing-out emulsions, its action being to accommodate the halogen liberated by light action by converting it into insoluble silver halide. This is again decomposed by light action, whereupon the accommodation again takes place, and so on, until sufficient silver is produced per unit area to yield a visible image.

Since in the case of negative emulsions the exposures usually given are very much less than that required to yield a printed-out image, it might be concluded that for this class of emulsion the presence of a sensitizer is not necessary. As far as normal exposures are concerned there is no need whatever for a sensitizer provided the halogen be allowed to get away from the silver, as the experiments of Daguerre, Herschel, and more recently those of Dr. H. Weisz (*British Journal of Photography*, 1907, p. 960) prove.

There is distinct evidence, however, that long before a visible image has been produced by the action of light on a gelatino-bromide emulsion bromine is liberated and its presence in the emulsion, as would be anticipated from theoretical considerations, retards the further decomposition of silver bromide. Thus a sensitizer confers printing-out speed on an emulsion. It does not necessarily affect the speed of the emulsion in the Hurter and Driffield sense. The H. and D. "speed" of an emulsion is quite distinct from, and probably bears no relation to, the printing-out speed. The H. and D. "speed" is always determined from the results obtained by exposure and development, and a moment's consideration will lead to the conclusion that development cannot legitimately be considered as equivalent to a continuation of exposure. In the first place development takes place under conditions (absence of actinic light) which should favor the reverse reactions of those instigated by exposure. Further, the reactions consequent on exposure may be considered as occurring in solid solution and only those portions of the emulsion components which are within molecular distances of each other can react. In a normally dry emulsion there can be little if any migration of silver atoms and the migration of gaseous halogen molecules must be comparatively slow. The conditions may be characterized as unidirectional, there being no evidence of reverse reactions.

In development, solution ionization and ionic mobility are normal and reactions direct and reverse can proceed in a normal way. Thus, if a gelatino-bromide emulsion be exposed to actinic light in a series of strips for successively increasing periods of time, deposits of silver of successively increasing density are produced up to the limit of the silver contained in the emulsion.

On the other hand, if the deposits be obtained by the combined actions of exposure and development, the ultimate densities of such deposits are governed by the concentration, composition, and time of action of the developer.

If the results obtained by exposure and development be represented graphically as a curve the abscissae of which are long exposures and the ordinates H. and D. "densities" (i. e., weights of silver) we obtain the well known curve which comprises three distinct branches, viz.: (1) a first ascending portion—the normal or first negative portion; (2) a descending portion—the reversal portion; and (3) a second ascending portion—the second negative portion.

It has been stated that Jannsen (*Comptes rend.*, 90, 1447; 91, 119) obtained evidence of repetitions of these portions, but although I have given exposures ranging up to 300 hours summer sunshine to a Kodak film emulsion no evidence of any such repetition was obtained on development (see also H. J. L. Rawlins; this *Journal*, 1891, p. 18). Failing further evidence, therefore, it may be assumed that the second negative portion is the last portion of the curve.

Using a film of pure silver bromide, i. e., free from a sensitizer, there is reason to believe that both the latter portions of the curve, i. e., the reversal and second negative portions, are absent. My experiments with a film of pure silver bromide are not sufficiently advanced to be presented as conclusive evidence, but as far as they have gone they confirm the results obtained by H. Weisz, who found that with such a film exposures long enough to produce a distinct printed out image yielded no reversal phenomena on development. As has been previously pointed out, exposures of very much shorter duration than are necessary to obtain a printed out image give distinctly reversed results on development where a gelatino-bromide film is employed.

These facts possess more than a passing interest to a student of the latent image in a gelatino-bromide emulsion. Hitherto, most of the work which has been done on this question has been concerned solely with the metallic element, to the neglect of any consideration of the influence of the non-metallic component (see, however, Trivelli, *Phot. Korr.*, 1911, 24, 182, and Lüppo-Cramer, *Z. Chem. Ind. Koll.*, 1911, 9, 22-25; also Lumière and Seyewitz, *Bull. Soc. Chim.*, 1908, 3, 743-750).

The evidence of the later workers seems to support the assumption that the latent image consists of colloidal silver in solid solution in unaltered silver bromide and of loosely combined halogen rather than of silver sub-bromide, Ag₂Br (K. Sichling, this *Journal*, 1911, 833, and Reinders, this *Journal*, 1911, 446, 927, 1231).

According to this conception the exposure of a

gelatino-bromide emulsion to actinic light causes a severance of the bond between the silver and the bromine. Both these components are in an ionized state and hence are chemically active.

The bromine attaches itself to the gelatin in some such way as indicated by H. R. Procter (*Chem. Soc. Trans.*, 1914, 313). According to this worker gelatin forms an addition compound of an ammonium type with the halogen acids. In this case the halogen acid results from the interaction of the bromine on the small amount of water present in a normal emulsion (a perfectly desiccated emulsion is insensitive). Nascent oxygen is also thus produced and this accounts for the tanning action which can be observed in an exposed emulsion. It appears probable that the tanning is effected by dehydrogenation and subsequent quinone condensation of the dehydrogenated products.

The severance of the bond uniting the metal and the halogen necessitates the application of a definite minimum amount of energy; exposures representing less than this minimum may cause a straining of the bond, but on the termination of the exposure normal, i. e., unaltered, molecules remain. In one branch at least of applied photography this inertia of the emulsion has to be considered; thus in stellar photography it has been found that telescopes having object glasses of relatively small diameter are incapable of producing developable images of the fainter stars, no matter how long the exposure, whereas a larger diameter objective produces developable images.

Adopting the nomenclature of E. C. C. Baly, it may be stated that the action of the light opens closed force fields of the silver-halogen molecule, and in order to produce a developable condition the respective atomic fields must be so far separated as to allow of their stabilization by interaction with suitable contiguous substances. The capacity to resist this action of the light is a measure of the true inertia of the sensitive compound which should not be confused with the apparent inertia as determined by the Hurter and Driffield system. This latter is determined from results obtained by the combined actions of exposure and development and for a given exposure can be modified by alteration of the development conditions or of the physical condition of the emulsion.

Thus, Sheppard and Mees have shown that, other things being constant, certain developing agents lower the H. and D. inertia of an emulsion, and it is further well known that using the same constituents in their manufacture a "ripened" emulsion exhibits a lower H. and D. inertia than an "unripened" emulsion.

Kinoshita (*Proc. Royal Soc.*, 1910, A 83, 432) has shown that if one molecule of silver bromide in any aggregate of molecules, or grain, is ionized, the whole grain becomes developable and therefore the mere aggregation of molecules which occurs in the ripening process is sufficient to account for the increase in the "speed" when such "speed" is determined from the relationships of the visible developed deposits.

It might at first sight appear that the presence of a reducing agent in the emulsion should, by the more rapid removal of the halogen liberated or ionized by exposure, decrease the inertia, but in this connection it is necessary, bearing Kinoshita's results in mind, to remember that the light energy necessary to produce a developable condition is exceedingly small in amount. Thus according to the calculations of Nutting the amount of light energy applied to an emulsion which on development will yield unit density (arresting 9/10 of the incident light) is of the order of 10^{-14} ergs per grain of 3μ diameter or 10^{-7} ergs per square centimeter of the emulsion. Provided, therefore, the halogen from one molecule per grain (of 10^4 molecules) can be accommodated the full "speed" of the emulsion may be obtained. The presence or absence of an energetic reducing agent in the emulsion can make no appreciable difference to the speed of that emulsion.

I have endeavored to show elsewhere (*Phot. J.*, 1914, 250 et seq., and 1915, 186 et seq.) that, as exposure is increased and the amount of halogen liberated is also increased, the presence of an active reducing agent in the emulsion modifies its properties appreciably. When the gelatin itself acts as the reducing agent by virtue of its power of combining with halogen acids, as before pointed out, it is quite efficient for all ordinary purposes.

With increased exposure the further liberated halogen

*Journal of the Society of Chemical Industry.

is mechanically held by the gelatin until such exposures as are sufficient to cause the expulsion of gaseous halogen from the film are employed. The development reactions under these varying exposure conditions can be the more readily described if we assume that a gelatino-bromide emulsion has been exposed in three strips; the first strip having received an exposure which develops to a density of unity, the second strip, such as develops to a reversed result, and the third strip, an exposure which yields the so-called second negative.

The development of strip (1) results from the electrolytic reduction of each grain in which ionization has occurred during exposure, the developed intensity accruing from the reduction of those molecules of silver bromide which were not affected by light.

In the case of strip (2) a small amount of silver—produced by the exposure—generally just discernible as a printed out image—is augmented by a small amount produced by a reaction similar to that occurring in the case of strip (1), but the concentrations of the oxidation products of the development base, and of the alkali halide, increase so rapidly by the action of the occluded halogen in the gelatin that rehalogenization of both deposits commences immediately and rapidly proceeds to a finish, when development is performed in non-actinic light.

This initial development and subsequent disappearance can be readily observed with suitably exposed emulsions when well diluted developers are employed. If development be conducted in actinic light the rehalogenization is never complete, in fact it has been shown that a developed reversed image may be redeveloped to yield a normal result after exposure to actinic light.

The reactions which cause the rehalogenization are in all probability exceedingly complex, but that the oxidized products of the developer are the cause seems certain. If, for instance, a small quantity of quinone is placed on a normally exposed emulsion and the image then carefully developed, complete reversal will occur in the areas which were in contact with the quinone.

In this connection it is of interest to recall the fact that Lumière and Seyewitz have made use of the combined action of an oxidized developing base and an alkali halide for the reduction of developed images (this *Journal*, 1910, 1228). The somewhat remarkable oxidizing power (by dehydrogenation) of bodies of a quinone structure has been commented upon by A. G. Green (*J. Soc., Dyers and Col.*, 1913, 100).

Strip (3). In this case the final developed deposit consists of the silver formed during exposure minus that rehalogenized by reactions similar to those occurring in strip (2). Obviously, the longer the exposure the denser will the developed result be, because once the film has taken up its full complement of halogen the remainder of the halogen passes into the atmosphere and is no longer available for the oxidation of the developer and hence for the rehalogenization of the silver.

The prevention of reversal is therefore dependent upon the effective accommodation of the halogen, liberated during exposure, in such a way that the developer employed cannot be oxidized by the reaction products of such accommodation. Various substances have been added to the emulsion for this purpose and as far as efficiency is concerned potassium nitrite, as recommended by Abney, is perhaps the most important, but its tendency to crystallize and cause reticulation of the film prevents its use in the case of the gelatino-bromide dry plate.

Other more or less readily oxidized compounds, e. g., manganous salts, quinine, and *m*-phenylenediamine, have been tried, but although they undoubtedly improve the results they do not react with the halogen quickly enough to prevent a large proportion of it from reacting with or otherwise being accommodated by, the gelatin; this, of course, largely annuls the beneficial effects of the special additions.

The bases used for development are among the most active reducing agents which can be used for the accommodation of the halogens and they can be used as sensitizers in a gelatino-bromide or chloride emulsion. With one or two exceptions, however, these developers cannot be employed in emulsions which have to be developed, as their oxidation products, formed during exposure, are, in conjunction with the alkali bromide, exceedingly active rehalogenizers. (The actual composition of the rehalogenized compound is at present unknown—it is completely soluble in a 10 per cent solution of sodium thiosulphate.)

Broadly speaking, the slower a developer acts in the normal way the less is the apparent reversing power of its oxidation products and hence these slow develop-

ers are applicable as sensitizers which greatly minimize solarization phenomena. Compounds of hydrazine and hydroxylamine have been successfully used in this connection (Eng. Pat. 1689 of 1908), but a serious diminution of the "speed" appears to result from their addition to rapid emulsions.

Highly beneficial effects are produced by the addition of one or other of the compounds derived from *p*-phenylenediamine, for the use of which, as preventives of reversal, a patent has been taken out by the author (Eng. Pat. 29,919 of 1912). *p*-Phenylenediamine itself may be employed, but the rapidity with which it is oxidized by air forming highly colored bodies which "slow" the emulsion renders it highly desirable to protect the amino groups. When these groups are so protected the "slowing" of an emulsion by the addition of one or other of the compounds specified in the patent is for all practical purposes quite negligible.

It is necessary, when using plates which have been rendered more or less immune from reversal faults, to develop with care if negatives showing detail in both high lights and shadows are desired. The reasons for this have been discussed more fully in a short communication to the Royal Astronomical Society (*J. Royal Astron. Soc.*, 1914, 24, [5], 261). In fine, the golden rule of "Expose for the shadows and develop for the high lights" with one or other of the "soft" working developers, should be followed. Ordinary dry plates may be either steeped in the diamine solution or the latter may be applied with a "Blanchard" or camel hair brush, and as alteration in the air is exceedingly slow the plates so treated may be stored under normal conditions until required for use.

A New Thermometer Scale

By Alexander McAdie

In the *Physical Review* (vol. VI, No. 6, December, 1915), I urged the importance of adopting without further delay, in aerophysics, units that have a scientific basis. At Blue Hill Observatory for the past two years we have published summaries of air pressure, also vapor pressure in kilobars or force units, temperature in degrees absolute centigrade, and wind velocities in meters per second. Wind direction is given in degrees, starting from the west; rainfall in millimeters and evaporation in force units.

The British Meteorological Office, under the progressive leadership of Sir Napier Shaw, has published since 1915 results in similar units. Unfortunately the European definition of the megadyne atmosphere, the bar so-called, is misleading, a more scientific definition being *megabar*. The use of the smaller unit does away with the inconsistency of defining a millibar as 1,000 bars, a self-evident contradiction, and allows us to define the bar, the basic unit, as the force which would impart to a gramme an acceleration of one centimeter per second per second. A *kilobar*, or 1,000 bars, becomes then the natural and consistent unit for all measurements of pressure in force units.

With regard to temperature, a concept all important in atmospheric, it is regrettable that we have no available method of expressing gain or loss of heat in terms of molecular motion. The ordinary mercurial thermometer is certainly a crude instrument, and so far as thermometer scales go, there has been no real improvement since Linnaeus reversed the order of the Celsius marking. It will be generally conceded that the Fahrenheit scale has now outlived its usefulness. Some meteorologists, however, still oppose the use of the centigrade scale and also the absolute, on the ground that the scale division even when read to tenths is too large for meteorological purposes.

To meet this objection and for other reasons, I suggest a new scale, to be known as the New Absolute, or briefly, New. The zero of the new scale will be the same as that of the absolute, approximately 491 degrees below freezing on the Fahrenheit and 273 degrees below on the centigrade. The other fiducial point is the temperature of melting ice at a pressure of 1,000 kilobars and is marked 1,000. (The degree sign is omitted as it has been decided to reserve this symbol for angular measure.) The scale divisions are thus 0.366 of the centigrade, even smaller than the Fahrenheit and permit of any desired refinement of reading. Some other advantages of the new scale are:

1. The abolition of all minus signs. In upper air work temperatures are far below freezing. At a height of ten kilometers readings may be lower than those recorded by Scott in the Antarctic. Given such a reading as -66.0 deg. Fahr., or -54.0 deg. Cent., or 219.0 deg. A.; on the new scale this is read 800. Or again, some of the surface winter temperatures are confusing unless the minus sign is emphasized. The new scale has an advantage here, as when we write 900

in place of -17.5 deg. Fahr., or -27.0 deg. Cent., or even 245.7 deg. A.

2. The grand division of *warm* and *cold* as experienced by the general public in the every-day affairs of life is characteristically marked. Warm is any reading above 1,100; cold is any reading below 1,000.

3. In published tabular work there is a saving in typographical composition. There is also a saving of time in computation and increased accuracy.

4. But of even greater importance is the fact that the new scale makes for clearer conceptions of the nature and magnitude of temperature changes. It is astonishing how indefinite and vague are the ideas of most students regarding heat and the significance of a given temperature. And, furthermore, it is a difficult matter with the present scales to present clearly to the student a picture of the process of energy transfer available as heat.

5. The new scale, starting as it does from the temperature of no molecular motion and laying stress upon the temperature of change of form of the most familiar substance, water to ice, has, it would seem to me, a certain educational value, which the Fahrenheit and centigrade certainly do not have and which the absolute scale, owing to the awkwardness of the fraction 1/273, loses.

6. Finally in problems of thermodynamics, there would seem to be no valid objection to its use. In the characteristic equation for a pure gas, the product of the new reading and the gas constant give as before a measure of the kinetic energy of translation of the molecules. Similarly, in Avogadro's law, we have an even more definite conception of the temperature function; and also in the Stefan-Boltzmann law of black body radiation.—*Proceedings of The National Academy of Sciences.*

Observations Upon Ants

M. CHARLES JOURDAN recently made some observations upon black ants (*Messor barbarus*) in Algiers which appear to show that this species is able to exercise spontaneous acts such as we call acts of judgment. In order to capture different birds, the author had placed small cages in his garden provided with grain boxes, and the ants soon made their appearance, so that to protect the grain he mounted the cages upon poles. But the ants then commenced to mount and descend the poles. However, they found this to be too much labor in order to carry off the grains, and the ants formed themselves into two squads, one of which stayed on top and threw down the grains, while the other picked them up and carried them off. He then coated the poles with viscous substance in which some of the ants stuck, but other ants came to the rescue and each one brought a grain of earth. In this way they soon covered the sticky surface with earth and were able to pass over as before. The author then had the idea of placing the cages on an iron tripod mounted in the middle of a large basin of water, and the ants wandered around it for quite a while. Soon, however, they returned in long files, each ant carrying a piece of substance such as a piece of a dead leaf or other light material, and soon covered the surface of the water so as to be able to cross over. Commenting on the above, M. Cornetz, whose researches upon ants are well known, says that during the life of the species such acts are repeated in such way as to enter into the class of automatism, though he does not make it clear how such acts originated. M. Ferton, an experienced observer of insects, states that the act of covering over a viscous substance is already noticed, and he found this to occur in various cases, and the same holds good for certain obstacles. However, the act of covering water with floating substances appears to enter into the class of "new" acts for which judgment needs to be used, for this species of ants never cross water in the usual circumstances. As to the method of throwing down the grains, this is surely unusual, but we should obtain more data on this point before discussing the matter.

Vaporizing Formalin

ACCORDING to a patent by the Schweizerisches Serum und Impf-institut Bern, 4a, Laupenstrasse, Bern, Switzerland, aqueous formalin for disinfecting purposes is vaporized by allowing the heat evolved by the hydration of dehydrated salt, such as dehydrated copper sulphate, to start the decomposition of potassium chlorate, which develops sufficient heat to start the vaporization. The constituents, including the aqueous formalin, are merely mixed all together, with the optional addition of manganese dioxide and pulverized metallic iron to act as a catalyst and to prevent oxidation respectively.

It is remarkable that direct halogen absorbers will neither prevent reversal nor serve as developers.

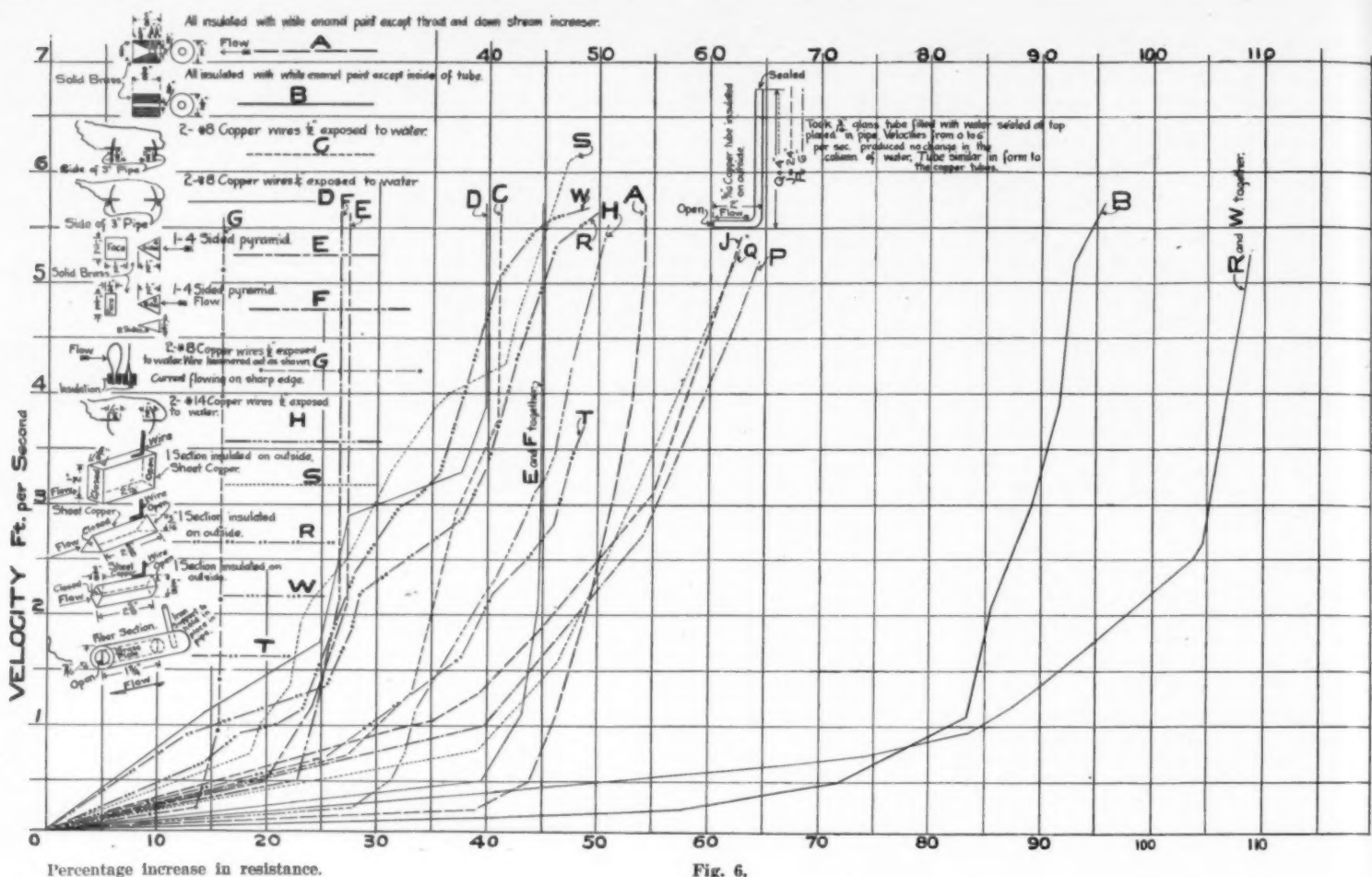


Fig. 6.

Can the Velocity of Water Be Measured

By Passing an Electric Current Through It

Walter S. L. Cleverdon, Assistant Engineer, Dept. Water Supply, Gas and Electricity, New York

With the apparatus which is at present on the market it is possible to measure the velocity of water and other liquids in mains very accurately, provided the velocity exceeds one half foot per second. If the velocity be less than this the accuracy is very uncertain, decreasing with the velocity.

With this in mind, the above question occurred to the writer, suggesting a possible method of measuring low velocities.

The following very rough experiment was made to determine if there were any grounds for such a question. Two rubber insulated wires were stripped for about two inches, fastened to a board about twelve inches apart and extended downward about six inches. This board with the wires was lowered into a flowing river. The shore ends of the wires were connected with four dry-cell batteries of 1.4 volts each, galvanometer and Wheatstone bridge, for the purpose of measuring the resistance in the electric circuit. This apparatus for measuring the resistance to the electric current was used throughout the experiments. Attempts were also made to measure the electric current with a voltmeter, ammeter and millammeter, but with no success. The board holding the wires was floated from comparatively still water to water with a velocity of about two feet per second and back again. Readings were taken on the Wheatstone bridge at the same time. These readings, which could only be taken approximately, showed that there was an increase in resistance to the electric current as the velocity of the water increased, and a decrease in resistance as the wires returned to the still water. These readings varied from 5,000 ohms in still water to 8,000 ohms with water flowing at two feet per second.

Traverses were next made on a thirty-inch water main with wires arranged as shown in Fig. 1. The brass pipe with the wires was forced through a stuffing-box fastened to a one-inch tap and into the thirty-inch main. On account of the low roof over the chamber in which these tests were made it was not possible to make the brass pipe holding the wires long enough so that it could be extended to a point nearer than two inches from the bottom of the thirty-inch main. The results

of one of the traverses is shown in the curve of Fig. 2.

The point on the main at which the traverses were taken was about seventy-five feet from the pump supplying the main. The pump took up considerable air on the suction, as an air cock was left open to "give the plunger something to cushion on." For this reason it was impossible to get readings when the wires were

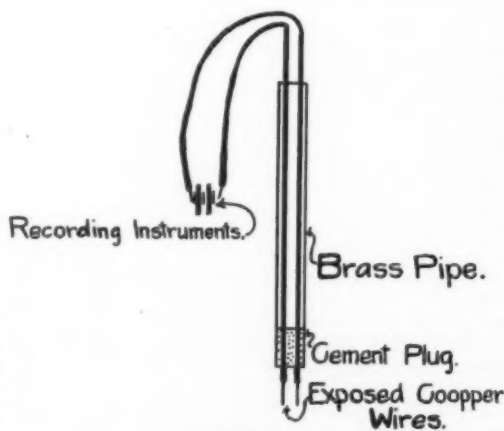


Fig. 1.

within two inches of the top of the main. The pump was down in a pit, forced the water upward into the main and then horizontally, which possibly accounts for the maximum resistance of the electric current taking place between six and eight inches below the top of the main, indicating a maximum velocity at that location rather than at the center of the main. It may also have been due to the increasing amount of air toward the top of the pipe. The mean velocity was about $3\frac{1}{2}$ feet per second, and the resistance to the electric current varied from 2,100 to 2,800 ohms.

These experiments were not made for the purpose of determining the velocity of the water in the main, but to determine roughly whether or not the curve show-

ing the resistance to the electric current would correspond to that of a pitometer traverse.

In order to experiment with low velocities, determine what effect water pressure had and the effect of the distance between wires inside of the pipe, a hose bib on the city supply was connected to a three-inch pipe which also discharged through a hose bib. By operating the hose bibs, velocities and pressures could be easily regulated (a pressure gage was kept on the three-inch pipe). The velocity of the water was determined by taking the time during which the water was flowing uniformly, and measuring the volume of water discharging into a large container. Rubber insulated copper wires were tapped into the sides of the three-inch pipe with about one eighth of an inch of wire exposed to the water.

Figs. 3, 4 and 5 give the location of the wires (poles) in the pipe and the results of the experiments. These figures indicate that a change in the velocity of the water up to one quarter foot per second produces a similar change in the resistance to an electric current. This is not so well shown where the velocity is being reduced as where it is being increased, which is probably due to the eddying of the water caused by the velocity of the water being reduced. Where readings were taken at longer periods after the velocity was reduced, the relation between the velocity and resistance to the electric current was more uniform.

The above experiments were repeated under water pressure varying from about one to fifty pounds, giving approximately the same results, indicating that the pressure had no effect on the results.

While the greater the distance between the electrodes in the water in the pipe produced a greater difference in resistance than when the electrodes were close together for the same velocities, this difference did not seem to be a function of this distance. It therefore seemed that the principal source of resistance was where the electric current left the wire and went into the water, or where it left the water to take the electrode, or both. This indicated that the shape of the electrode extending into the water might affect the result, suggesting experiments along this line. Up to

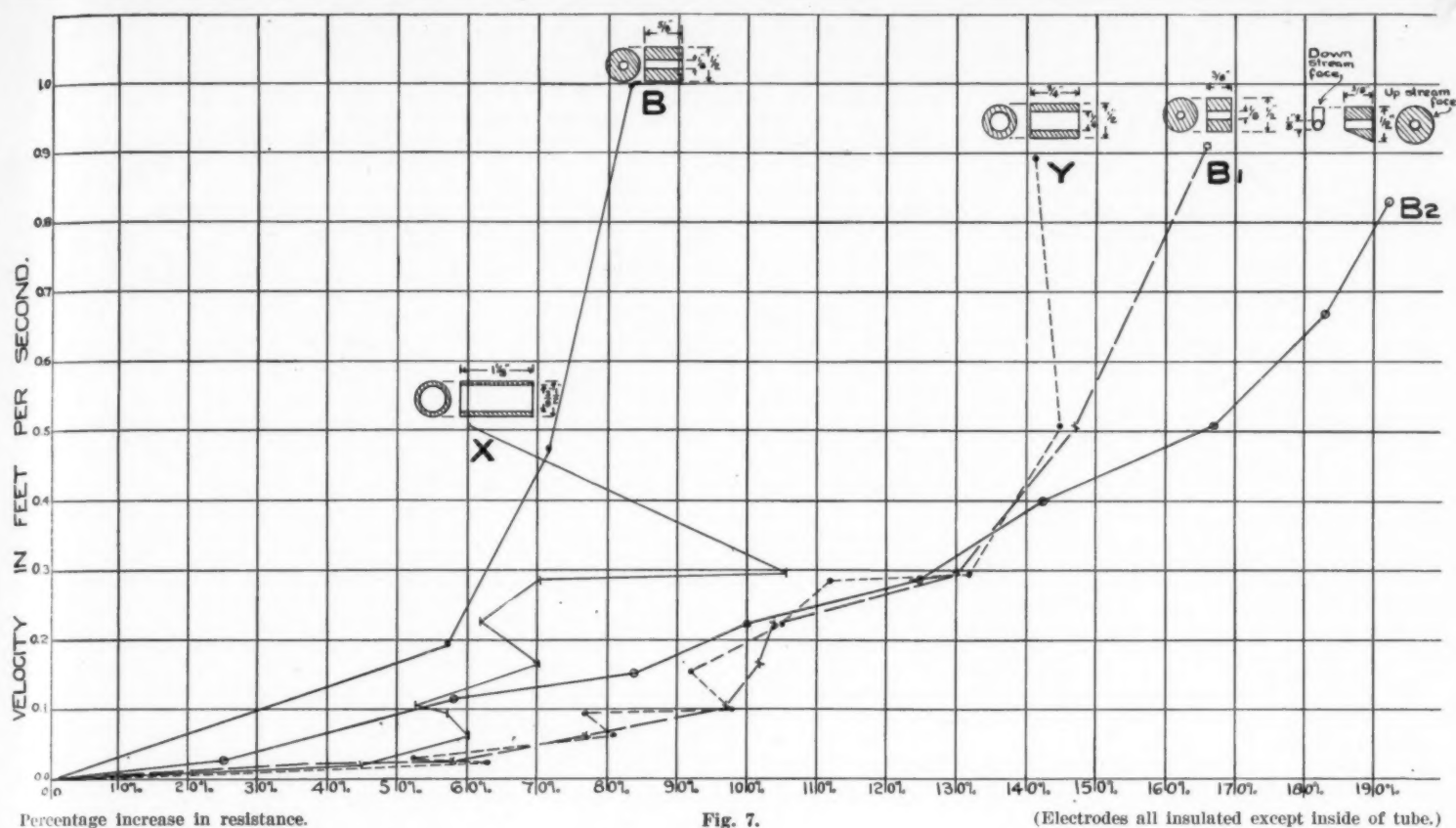


Fig. 7.

(Electrodes all insulated except inside of tube.)

this point wires were the only electrodes exposed to the water and extended into the water perpendicular to the line of flow.

TESTS TO DETERMINE THE EFFECT OF THE SHAPE OF THE ELECTRODE ON THE RESISTANCE TO THE ELECTRIC CURRENT.

The hydraulic apparatus used in these experiments consisted of a tank ten feet long, three feet wide and three feet deep, a centrifugal pump, Venturi meter, a gate on the discharge side of the pump and piping. Water was drawn from the tank by the pump with three-inch suction, discharged through a three-inch main into the meter and then returned to the tank through a three-inch pipe. With this apparatus it was possible to obtain velocities up to about six feet per second.

As a preliminary experiment one wire (pole) was placed inside the pipe while the other was attached to the outside. The outside of the pipe was well cleaned with a file before the wire was attached. Readings

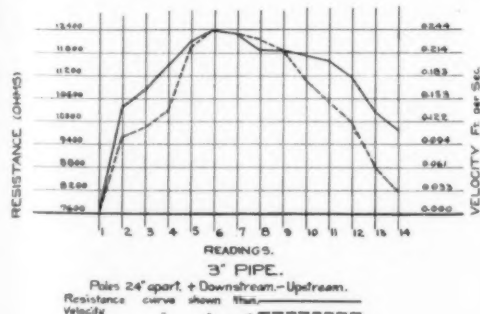


Fig. 3.

were taken on the Wheatstone bridge with water passing through the pipe at different velocities. Both wires were then placed inside the pipe, the same velocities repeated as in the first experiment, and readings again taken on the Wheatstone bridge while these velocities were maintained. It was found that the resistance to the electric current in the first case was approximately one half that in the second case for the same velocities. (See Fig. 6.) An examination of the curves obtained with electrodes R and W, the curves obtained with electrodes E and F, and the curves obtained by using pairs of these electrodes RW and EF together, show this condition approximately.

In order to test each form of electrode it was therefore only necessary to use one of each to give comparable results. Fig. 6 shows the results of these tests and would seem to indicate that: Where the electrode is long and narrow like a knife blade and points in the direction of the flow of the water there is very little change in the resistance to an electric

current for any velocity of the water, the water keeping constantly in contact with the electrode over its entire length. (See electrode G.) Where the electrode is wider and suddenly terminates there is a tendency for the water to leave this portion of the electrode, forming a partial vacuum at this point, reducing the area of the electrode in contact with the water, thereby increasing the resistance to an electric current.

With this condition reversed, as shown with electrodes E and F, where the water was flowing on the base of the pyramid and left at the point, the water seemed to keep in contact with the electrode and conditions for increasing the resistance are shown to be little better than with electrode G. Electrodes were therefore made with the object in view of forming a

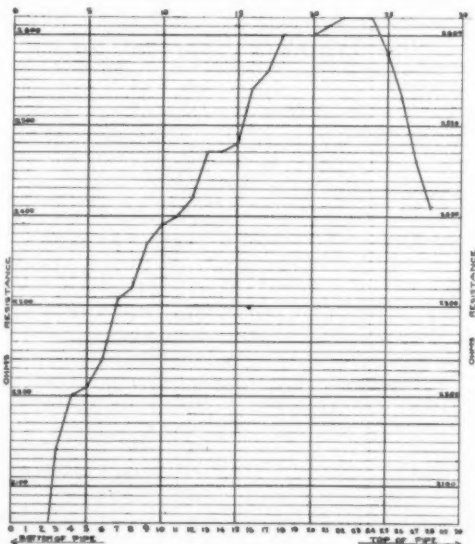


Fig. 2.

partial vacuum between the electrode and the water as the velocity increased, thereby increasing the resistance to the electric current. Electrode B (see Figs. 6 and 7) was made with the idea that the contraction of the water as it entered the electrode would produce this condition.

The following is copied from Merriman's "Hydraulics," page 128:

"A standard tube is a very short pipe, whose length is about three times its diameter, or of sufficient length so that the escaping jet just fills the outer end and there issues without contraction."

This suggested that the length of the tube B be reduced from five eighths inch to three eighths inch, i. e., to three times its diameter, in order that the water as

it passed through the tube gradually contract and leave the uninsulated portion of the electrode as the velocity increased. The results are shown in Fig. 7, electrode B, giving an increase of about eighty per cent in the resistance to the electric current. The curve representing these results is seen to be rather irregular at a point where the velocity was about 0.2 foot per second. The downstream side of the electrode was reduced as shown in electrode B2, which not only gave quite a smooth curve, but an increase in the resistance to the electric current.

Electrodes X and Y are made with their lengths equal to three times their diameters, but there probably was not sufficient surface on the upstream side to give proper contraction to the water. The curves representing the readings taken with these electrodes are very irregular, indicating that the water passed through the tubes in an agitated condition, and with no regular contraction.

In making the above experiments water from dif-

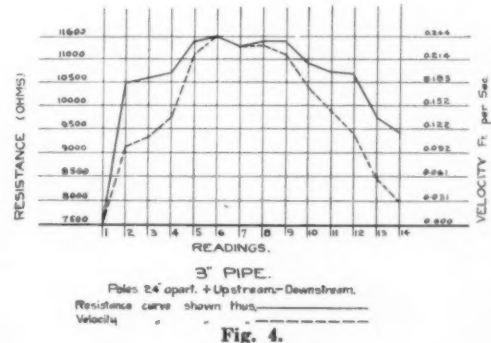


Fig. 4.

ferent sources was used, and the difference in resistance which it offered to an electric current was quite marked. A sample of Croton water was placed in a glass jar with electrodes extending a certain depth into it and held rigidly apart. The water was allowed to become quite still. The resistance to an electric current was measured. The Croton water was then removed and replaced by the same amount of water from the Ridgewood Pumping Station, Brooklyn; the resistance to an electric current was again measured as in the first case. The Croton water offered 34 per cent more resistance than the Ridgewood water. The Croton water is surface water while the Ridgewood water is from wells which contain iron and other salts.

EXPERIMENTS TO DETERMINE THE EFFECT OF ADDING SALT TO WATER.

Experiments to determine the effects caused by adding salt (NaCl) to water on the resistance to an electric current were then made. The same apparatus was

used as above described. Croton water as drawn from the tap was first used.

The same electrodes were left in the pipe throughout the experiments. Readings of resistance were taken with still water in the main, and again with the water flowing at certain velocities. Salt was then added to the water in the tank, the flow continued until the same became well dissolved and mixed with the water. Readings of resistance to the electric current were again taken under the same conditions of velocity as in the first case, more salt was again added and readings repeated. The results indicated the following proportion:

Let R represent the resistance to an electric current with still water, and O the resistance of the same water under a velocity V .

After adding salt, let R' represent the resistance to an electric current with the water still, and O' the resistance to the electric current with a velocity V .

This operation was repeated by adding more salt and in all cases the readings indicated the following proportion:

$$R : O :: R' : O'$$

One half a bag of salt (about two pounds) reduced the resistance in the still water from 5,100 ohms to 930 ohms, and with a velocity of 4.3 feet per second from 7,200 ohms to 1,300 ohms.

These figures satisfy the above proportion within one per cent.

DISADVANTAGES.

The slightest change in the quantity or composition of the salts held in solution will greatly affect the conductivity of the water. The addition of a slight amount of salt will increase the conductivity.

The slightest amount of grease or oil coming in contact with the electrodes will greatly increase the resistance.

The electrodes must in all cases be kept clean.

The slightest change in the distance between the electrodes will produce a change in the resistance. For distances less than one inch this is very pronounced, but decreases as the distance increases.

Air in the water increases the resistance as it comes in contact with the electrodes.

ADVANTAGE.

The slightest movement in the water is recorded by an increase in resistance.

CONCLUSIONS.

The results obtained from the experiments would

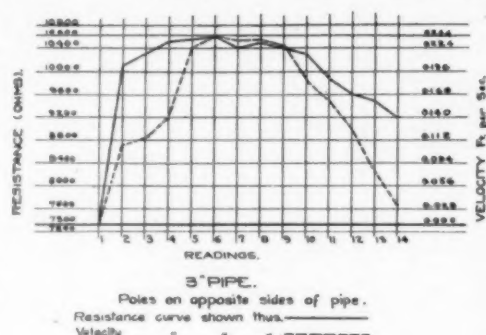


Fig. 5.

seem to indicate that velocities of less than one foot per second might be measured quite accurately for short periods only, and that the slightest movement in the water may be detected, provided the electrodes used were first standardized by determining a percentage curve of increase in resistance covering all velocities, from O up to the maximum velocity encountered.

tered in the mains. Then determine the resistance which the same electrodes would give in the water to be tested when it is still. The increase percentage in resistance due to the velocity will give the abscissa, from which point the ordinate will intersect the velocity curve giving the required velocity.

Velocities greater than one foot per second could only be approximated with the electrodes used in the above experiments, the increase in resistance being very slight for increasing velocities.

With a suitable instrument for measuring the resistance in the circuits, or measuring the electric current, this method might be used to advantage in determining roughly, but very quickly, the approximate velocity in a main, and the slightest movement in the water.

In streams where the velocity is so slight that current meters will not register, this method might be employed.

If this method could not be developed so that high velocities could be measured, it might be used as an auxiliary with some other instrument recording velocities lower than are at present recorded.

On account of the disadvantages given above the writer does not believe it practical to use this method for giving a continuous record of flow. This would be especially true in measuring the flow of sewers or water which had been chemically treated. It would, however, seem practical to apply this method with a suitable recording instrument where it is desired to determine quickly whether or not there is any flow in pipe lines, conduits, etc., when looking for leaks, as the slightest flow is quickly shown by an increase in resistance.

The author desires to acknowledge with thanks assistance rendered him by Prof. Wm. R. Bryans, New York University, and John F. Cleverdon, electrical engineer, New York, in carrying out his experiments.

Vibrations, Waves and Resonance—III*

The General Principles Underlying Wave Motions

By J. Erskine-Murray, D.Sc., F.R.S.E., M.I.E.E.

Continued from SCIENTIFIC AMERICAN SUPPLEMENT No. 2143, page 64, January 27, 1917

In my last lecture I showed you that musical tones and articulate sounds are merely certain types of continued vibrations, and since the waves given out by them are really the same vibrations in motion from place to place these waves have the same characteristics. The strange thing is that, although each set of waves is independent of the others in the sense that it travels on its course undiminished and undistorted by the others it may meet, the actual vibration at any particular point, at the drum of your ear, for instance, being the sum of all the pressures and rarefactions arriving at each moment, is in itself merely an alteration of pressure and rarefaction—not a simple harmonic vibration, of course, but a harmonic variation composed of many simple components. It can, therefore, be represented by one wavy line, apparently irregular, but in reality composed of many simple harmonic motions. This one line represents a motion which is very simple as regards space; it is in fact merely backward and forward in one short straight line, but may be infinitely complex in time. Thus it is that the little wavy scratch on a graphophone disk or the tiny pitted groove of a phonograph may be the completed record of the sound waves of a band of many instruments, of a single human voice, or of the confused babel of a market-place. There is no further mystery in either of these instruments. Inside the box there is only ordinary clockwork, and the reproducer is merely a fine needle attached to a little glass or mica disk. The mystery of the music is wholly in the wavy line. Once you have got some way of making a piece of material large enough to cause audible compression and rarefactions in the air, to move according to the wave forms frozen on the record, you can obtain again the sounds they represent. Here is an ordinary graphophone record rotating at its proper speed. I hold a thin calling card with one corner in the groove. As the record turns the card is forced to vibrate with the wave forms, which have been impressed on the groove, and so gives out the sounds—faint, no doubt, for the card does not take so firm a hold as does the needle of the reproducer, but quite clear and distinguishable.

Do not forget that in the air and even at the drum

of your ear a sound is nothing but alternate compression and rarefaction, or a mere to-and-fro motion—the music is in your mind alone; it is a picture painted, not in space and color, but in time and motion, and, like other pictures, pleases or displeases by the conception of harmony which it gives. It was something of this sort that Fourier meant when he said that if one could comprehend at once the laws of the diffusion of heat it would give one the sensation of music.

It might seem a misuse of terms to call the grinding shriek of a tramway car as it turns a corner a musical note, but though unlovely it has something of that quality, and so must be caused by repeated vibrations. To those unaccustomed to associate such sounds with the presence of a uniform vibration the curiously regular flutings so often observed on the surface of tramway rails remained a mystery, and were provocative of all sorts of far-fetched explanations which merely confused the issue. A long discussion on "roaring rails" on railways went on at intervals during many years in our premier engineering institution, while, oddly enough, at the same time the subject of the "corrugation of rails" was chronic in another. Internal evidence from the proceedings of both societies shows that the phenomenon, though known by different names, was one and the same, although the aspects of the two societies were different. In the tramway case it consisted in the appearance of certain bright patches on the surface of an otherwise neutral-colored steel rail—patches which indicated the development of peaks and hollows on it, which were certain to lead to its early suicide, causing much grief and expense to its masters. On the railways, on the other hand, it was the noise that attracted attention. Each time a train ran over certain sections of the line it roared as if the brakes were on, and worse sometimes. Passengers complained, and the careful permanent way engineers were even more annoyed than the noisy oyster. I first became interested in the phenomenon when travelling daily from Bushey to Euston in 1907. After studying the rails at various parts of the line, and obtaining permission of the company, I went out one fine Sunday with a genial foreman platelayer armed with some long rolls of tracing paper about two inches wide to obtain tracings of the markings on the rails which roared.

These direct tracings have been transferred to this wall diagram on their full natural scale, and although they were done eight years ago, you are the first audience to see them.

At first sight the markings appeared to be hopelessly irregular, and gave no clue to their origin; but on looking more closely I found a characteristic form repeated itself with slight variations at definite intervals—an almost certain sign that the corrugations had their origin in vibrations. Look, for instance, at the rail marked (A). First you see a row of small bright patches at nearly regular intervals of about an inch; then follow three or four longer and less regular marks; and this complex pattern of smaller and larger markings is repeated again and again along the rail at intervals of about two and a half feet. Now, the longer markings occur on the portions of the rail near its point of support—the chairs—and the shorter ones in the interval where the rail is free: thus every time a wheel passes over the rail it dances along the unsupported part, and then skids in longer jumps up the minute slope to the chair and over it, repeating the whole performance at every successive sleeper. I have observed miles of this pattern on the London & North Western Line and on others of our best-laid express lines. This observation at once disposes of the suggestions which have been made that the corrugations might be due to soft spots in the metals or to uneven rolling during manufacture, for it is quite beyond all probability that the rails should be soft at regular intervals corresponding to those between sleepers, and then in so complicated but definite a pattern.

I have also observed that "roaring" rails on railways and "corrugated" ones on tramways occur almost always in places where the conditions are unusually favorable to the production and repetition of a particular vibration. Such conditions are: (1) a tendency for the wheel to slip intermittently instead of rolling, i. e., a moderate tangential force between wheel and rail, so that the wheel acts like a bow on a violin string or the exciting stick on one of Gould's bars; (2) a repetition of these conditions of motion for every wheel passing the section of track. Thus the worst corrugation in tramways is generally found on curves, and on these both conditions (1) and (2) exist, for (a) the

*Cantor Lectures before the Royal Society of Arts. Republished in the *Journal of the Society*.

difference in distance traveled by the outer and inner wheels on any axle in rounding a curve involves slipping of the wheels, and (b) the fact that there is very rarely a stopping-place on a curve insures that every car goes over the section at nearly the same speed, unless, indeed, the other traffic is so heavy as to cause frequent delays. Conditions (1) and (2) occur also just before and just after stopping-places, and there also one often sees corrugation, even where the line is straight.

On railways I find that the same conditions are fulfilled on the sections giving most trouble. Where the rolling-stock of nearly every train is of the same type and the speed nearly the same for all trains, it is clear that the vibrating masses and their motions are alike; and I find that it is just in these circumstances that the markings which indicate the roaring rail appear most frequently. Thus on the fast lines of a four-track main line, and particularly where one express train of long bogie carriages follows another at almost exactly the same high speed with the brakes on, corrugations often occur, though these do not appear on the slow lines in the same section. On the latter the rolling-stock is of many descriptions, from passenger coaches to all types of goods and mineral wagons, and the speeds are as various; on them, therefore, the conditions for a definite vibration and its repetition are absent. If a corrugated rail be moved to another section of the track it is not uncommon for the markings to disappear, which, of course, is a further proof that the corrugations are due to a combination of circumstances favorable to vibration and not to irregularities of manufacture. The consideration of a large number of observations on tramways and railways in various parts of the United Kingdom has thus led me to the conclusion that roaring, or corrugated, rails are initially caused by vibration, and, once commenced, the corrugation naturally increases with time, since every wheel that passes deepens the hollows and renders more energetic the vibrations of all that follow.

To-day our subject is "Ether Waves," that is to say, the waves which are called electric, and those of light and heat. Owing to necessary restrictions in time of war I am unable to experiment with the longer waves such as are used in radiotelegraphy; but this may, after all, be a blessing in disguise, for, as ether waves travel at an enormous speed and are usually invisible and always inaudible, it is really better that I should have to demonstrate their laws and properties by mechanical illustrations which you can see and appreciate. These illustrations are not mere analogies, but are simply other examples of the same law. When, for instance, I show you the attenuation of a wave, it is in illustration of an important law of wave motion, and not merely of a particular phenomenon in submarine-cable signaling, telephony or fluid motions; the demonstration is, in fact, a moving picture in which the one thing conspicuously obvious is the law.

I shall not attempt to go into the question of the molecular nature of electricity since it hardly concerns us in a general survey of wave motions, and shall speak of charges of electricity and their conduction as mere facts, without giving an electronic or other explanation. I do not mean to imply that the theory that electricity moves as a number of small discrete particles is untrue, but only that for our purposes the ultimate nature of its structure is unimportant, the mass result being that with which we deal.

Now I must say something which may appear paradoxical but which is nevertheless true. An electric wave is a moving state of stress in the ether which may cause the motion of electricity in a conductor, but is not itself the motion of an electric charge. For some purposes it is convenient to look upon this changing electric stress, or displacement as Clerk-Maxwell called it, as an electric current, but it is not in itself the motion of a charge of electricity; in fact, completely closed moving rings of it may exist in a medium which is wholly non-conducting. There are, therefore, both free electric waves which travel unguided through a non-conductor, and conducted waves which are guided by a conductor. The electric stress is of the same kind in both cases, but the presence of a conductor profoundly modifies the nature of the wave motion. Suppose that we create a series of electric waves, the wave-length being a few meters as in Hertz's experiments. Such waves in uniform free space will travel in straight lines outward from the point of origin; but attach them to a conductor and they will follow it even round the convolutions of a coil of wire a few millimeters in diameter. Again attach them to the earth, as at a wireless station, and they will follow its surface over hill and dale and over the waves of the sea. I do not say that a mountain in their path makes no difference, but experience has shown that it in no sense casts as perfect a shadow as it would do

if the waves traveled in straight lines, even allowing for the inward bending of the rays usually called diffraction. We must not, however, consider a conducted wave as something essentially different from a free one; it is not, it is just literally a wave conducted, i.e., guided by the surface of the conductor.

In the material of the conductor itself the wave has no longer the simple nature it has outside. As it penetrates the surface it generates conduction currents, and energy is used up in driving these. Also the better the conductor the less stiff is the dielectric in it and the smaller the depth to which the waves penetrate. In a good conductor, such as copper, the generation of the current commences very evidently at the outside, and when a constant E.M.F. is applied along the surface it is, comparatively speaking, a long time before the flow becomes uniform, even to a depth of a few centimeters. The process is in reality one of diffusion, not radiation, and the establishment of the steady state of the current is a matter of several seconds. Thus it is that we have what is called the "skin effect," that is to say, the limitation of the major part of an alternating current to the outer layers of a conductor, i.e., to those in contact with the dielectric.

It is important that you should realize the fact that the energy in electrical actions travels *via* the dielectric or non-conductor. This is quite clear when we are dealing with free radiation, but not so obvious for conducted waves or for the limiting case of an infinitely long wave, which we call constant, or direct, current. It has been proved mathematically by the late Prof. Poynting, in an investigation which is fundamental to modern theory, but which is too elaborate to give you here. Instead, I shall ask you to consider a simple experiment from which the same deduction can logically be drawn. Suppose that I am in a small room with continuous walls of solid copper a foot or more thick in which there is no crack or opening whatever. Outside the room there is a dynamo or other source of electric power; inside I have an ordinary electric lamp which I wish to light from the power outside. How can I do it? There is no use soldering both terminals of the lamp to points on the inside of the walls and those of the dynamo to points on the outside, for the walls are thick copper and the difference of potential between any two points on the outside, and still more between those on the inside, will, as you know, be utterly infinitesimal. No, if I want to get the energy in there is only one way to do it, and that is to bore a hole through the copper and run an insulated wire through it to the lamp. The material of the insulation is of little consequence, the essential point being that there must be a continuous tube of insulator from the dynamo through the wall to the lamp. This is a direct proof that the energy travels *via* the insulator, and it is at once simple and logically convincing.

It is for this reason that the stresses, whether constant or varying, in a dielectric are of so great importance: they are the means by which electrical energy moves from place to place and fulfils its manifold functions; a conductor may guide, but it does not convey and without a dielectric is useless.

A similar law holds in mechanical things. Suppose we have a large pond into which a jet of water is projected horizontally from a pipe at one end. No appreciable energy will reach the far end, even though the flow from the pipe be of considerable velocity. But, if instead of allowing the jet to diffuse itself and mingle with the mass of water in the pond, we contain it by making it flow through a hollow cylinder of some material which can permanently support an elastic stress; if, in fact, we continue the pipe across the pond, we shall obtain the energy at the far end in almost its full amount. The pipe, or insulator, has conveyed it along its solid walls.

In order to eliminate the mysterious force called gravity, I have supposed the pipe to be horizontal and uniform in diameter; this being so, there can be no steady flow of water in the pipe unless there is a tension in the walls of the pipe, and more than this, there can be no flow unless this stress decreases along the pipe in the direction of flow.

The flow of electric current in a submarine cable has many points in common with that of water in a pipe. The dielectric, for instance, is a hollow cylinder filled with, and surrounded by, a conductor just as the pipe is surrounded by and filled with a fluid, while its thickness and its electrical rigidity, together with the resistance of the conductor in its core, control the amount of current under any given difference of pressure between its ends in the same way that the flow of water in the pipe is controlled by the rigidity of the walls and the viscosity of the fluid. Indeed, so similar are the phenomena when considered as questions in the flow of energy that it is possible to employ the one as a useful working model of the other.

On this horizontal board I have laid a thin-walled rubber tube of four feet or five feet long; it is filled with a mixture of syrup and water, and you can see a drop hanging from the far end over the bowl. At the near end the tube is fixed on to the jet of a strong syringe, also filled with the same liquid. Now I press the piston of the syringe firmly and raise a bulge in the tube. The swelling creeps along, but not till it comes to the far end of the tube does any liquid run out. Now this is a true picture not only of the flow of electricity in a submarine cable, but also of the flow of water in any level pipe. In it, however, I have chosen the dimensions and materials so that the time factor is, as it were, magnified. Where the bulge takes several seconds to travel along the rubber tube, it would have flashed from end to end of a steel pipe of the same size in a minute fraction of a second; also, it would not have been of visible size, for steel is so much stiffer than rubber; but it would have happened all the same. In an electric cable of the same length as the tube the motion would have taken place even more rapidly, indeed the tube represents in this respect a cable of several thousand miles in length.

Note that the moving bulge is a true wave or impulse, not merely a mass of material moving forward, and that in fact the liquid which I put into the tube from the syringe remains at this end, and that the movement of any particular drop of liquid is merely a small distance forward and outward as the pressure rises behind it. The pressure travels the whole length of the tube, but the liquid only takes one short step. We have, therefore, a true wave or impulse swelling along the tube. To make the phenomenon obvious to those of you who are not close at hand, I have mounted a number of little flags on wires which are attached to the board at their lower ends and lie across the tube. You see that each flag rises as the bulge reaches it, and that it falls again shortly after. Now this model is very like the electrical case. The elasticity of the rubber walls of the tube is the capacity between the core of the cable and the sea water outside it; and the resistance due to the viscosity of the liquid is the electrical resistance of the core. In neither case is the inertia important, as the motion of the liquid is so slow that it is almost completely dependent on resistance of the liquid and elasticity of the tube, while in the cable the only factors of serious importance are the capacity and resistance. To follow the similarity you have only to recollect the picture I made for you in the first lecture, in which I showed the action of a condenser in terms of an elastic solid in which were cavities containing liquid. In this case, however, the solid is not an infinite mass but is merely a hollow cylinder surrounded by, and filled with, liquid. The greater the capacity, i.e., the thinner the walls of the tube, the slower does the impulse travel, and increase of resistance, whether fluid or electrical, produces a similar effect.

The frequency of the waves used in cable telegraphy is about ten per second, and, as the time that an impulse takes to travel from Ireland to Newfoundland is a large fraction of a second, there are several waves on their way along an Atlantic cable at one time.

Waves of all sorts gradually die out as they progress through a medium along a conductor, and short ones do so more rapidly than those of greater length. Technically this decrease in size is called attenuation, and when a compound wave travels its shorter components become more rapidly attenuated than the longer ones. Thus, in a telephone conductor the higher harmonic waves which define the vowel or consonant sounds, as I explained to you in the first lecture, are more reduced in proportion than the fundamental wave. The result is that, although a voice may sound quite loud at the far end of a long line, it may have lost its character, and it may be difficult to distinguish between such sounds as "ee" and "oo". This is the reason why as yet telephony has never been carried out through submarine cables of any considerable length, for in a cable the largeness of the capacity, providing, as it were, a shunt circuit to the sea, causes great attenuation. Even in underground cables, such as, for many good reasons, we have in London, the capacity is much greater than between the wires of an overhead circuit, and it is possible that this is one reason why the telephone operators are instructed to roll their "r's" in a triumphant "thr-r-ree," instead of pronouncing them in the ordinary way. One would not naturally expect that "three" and "two" could be confused, as they sometimes are, but when one considers the wave forms of "ee" and "oo," one understands the reason. The sound "oo" is practically a perfect sine wave, while "ee" is a similar wave with five small ripples on it. If these disappear there is only "oo" left, and "three" becomes very like "two."

Now I shall show you the attenuation of waves and

the distortion, or loss of character, of a complex wave as it progresses. Here is a long piece of the light fabric called butter-muslin, with wooden stretchers across its ends, one of which is attached to the picture-rail on the wall. Holding the stretcher at the free end of the muslin in my hands, I pull it gently toward me until the fabric is pulled up almost to a horizontal plane. Now I move the end of the muslin slowly but steadily up and down, and you see large waves traveling slowly along it toward the wall, which they just reach. Next I make the up-and-down motion much more rapid, and although I am actually giving out more energy than I did when making the long ones, the short waves produced die out long before they have traversed the length of the muslin, and, indeed, are quite imperceptible beyond a distance of a yard or two; thus the attenuation of the short wave is very much greater than that of the long. Here you have the reason why long waves are preferred to short in long-distance telegraphy of all kinds.

If now I send out both types of wave at once by moving my hands up and down with a rapid vibration superposed one a slower one—in what, for want of a better term, I may call a series of jerks—you see that the compound wave which goes along the muslin soon loses the ripples which give it its peculiar character, and long before it reaches the end becomes quite smooth. There you have an explanation of the difficulties of submarine telephony and, indeed, of all long-distance transmission of complex waves. Even in ocean waves the same effect is to be seen when a long, oily wave comes in over a glassy sea without a ripple on it.

There is one way to reduce the attenuation of short waves along a wire or of pulses along a cord, and that is to add inductance or inertia uniformly distributed along it. Each wave then represents a greater quantity of energy, and as the transverse motion is more rapid in a short wave than in a long one of the same amplitude, the energy is proportionately greater in the short wave than in the long one. Hence the short wave is fortified against the attenuating influence of resistance and capacity, and the complex wave travels on with less distortion. For this reason one more easily obtains "harmonies" on the wire-covered strings of a violin or guitar than on the lighter gut strings, and better articulation on a telephone cable loaded with inductance than on one with a single straight conductor. The Channel telephone cable, for instance, has extra coils of wire inserted in it at short equal intervals along it in order to make speech to Paris possible, and the same arrangement is used in some of the very long land lines in America and elsewhere. The distances between the coils must be short as compared with the lengths of the waves transmitted, so as to approximate as nearly as possible to a uniform distribution of inductance. Adding inductance, of course, reduces the velocity of the wave along the conductor, but as the energy is not seriously decreased this is of much less consequence than the improvement in the clearness of speech.

If a coil of very great inductance is put in series with an alternating current or telephone line, it may almost entirely prevent any current flowing. Large inductance means large storage of energy as magnetic lines of force in the medium surrounding the conductor, and the actual reflection back along the conductor of much of the wave energy. The introduction of a large inductance in a line in which the distributed inductance is small, therefore, cuts down an alternating current, just as an increase of resistance would do; hence, in technical parlance, such a coil is usually called a "choker." I can show you the same wave effect on this long-stretched rope, at a point on which I have fixed a seven-pound weight. I give one end of the rope a vigorous waggle, but although the wave runs rapidly along as far as the weight it gets no farther, but returns toward my hand. Here the mass has so great inertia that the force of the wave is not sufficient to set it quickly in motion, and the rope swings over and returns before it has moved appreciably. Thus the wave is reflected and returns to the origin. In the electrical case the wave has not time to overcome the inertia of the choking coil, due to the large amount of magnetic force which has to be built up during even the smallest increase of current, and its energy is therefore also reflected back to the origin.

At first sight this phenomenon would appear to contradict the law that loading the line reduces the attenuation of waves, but it is not so. The difference lies in the distribution of the inertia. If this be added along the whole line or in small lumps commencing near the origin, there being a considerable number of lumps to each wave length, a wave starting with the same energy is more persistent, for its energy is now largely magnetic, and therefore is not wasted but

is returned to the current at each half oscillation. A smaller wave current has therefore the same energy as a larger one on an unloaded line, and since it is smaller there is less loss due to resistance, and, therefore, less attenuation of the wave, as it proceeds.

It is a general rule that when a wave passes from a less dense to a more dense medium, some part of its energy is reflected back into the less dense medium, and that if the difference in density is very great the reflection may be nearly complete. Thus, when a wave travels along the cord to the end which is fixed to the wall, it is almost entirely reflected. There is no doubt a wave of very small amplitude in the wall itself, but, owing to the mass being so great, it is practically negligible. It is also true that when a wave goes from a stiff into a slacker medium or vice versa, there is partial reflection. If, for instance, I strike one end of a beam with a hammer, the compression wave goes along to the far end and starts a wave in air which you hear as a sound, but a good deal of the energy comes back in the beam to my end, and is again reflected internally. I can tell that this is so from the mere fact that the beam gives out a definite note, which proves the existence of a succession of waves, although I only gave the beam one stroke.

To eliminate the difference of density one might immerse the log in a liquid of its own density but of a different compressibility, which is not difficult to do, and the result would be similar to that in air.

I have lately noticed a curious case of the production of a musical note by reflection which appears to be analogous to that which one gets on clapping one's hands in a room with bare walls. If one is standing on smooth ground while an aeroplane is passing overhead one hears a rough musical note of fairly definite pitch. On bending down, however, the pitch rises, and if one lowers one's head to about half its usual height the note goes up a whole octave. The effect is very curious, and is particularly marked if one keeps on bowing continuously. A similar sound can be heard, though less clearly, under a tree whose leaves are rustling in a breeze. I noticed it first under an aspen in my garden on a quiet afternoon. The explanation seems to be that each impulse reaches the ear twice in quick succession, once while going down and once when rising after reflection from the ground; thus, though the original series of impulses is probably irregular, particularly from the rustling tree, the fact that each gives, as it were, a little double knock to the ear in passing and re-passing, and that the time interval between the knocks of each pair is the same, gives one the sensation of a musical note. It is curious to think that these definite musical tones of the forest, though heard by countless generations, do not appear to have been recognized or recorded, except perhaps in a general way by some nature poets.

Hertz's first experiments on the production of electric waves were complicated by the very same phenomenon. It is known now that when at the outset he thought he was tuning his receiver to the waves from his radiator he was, in fact, tuning to the interval between the direct passage of an impulse and its return reflected from the wall. The frequency observed was, in fact, like the hum of the aeroplane, dependent on the distance of the receiver from the reflecting surface.

Here I may remind you of one great generalization made by Clerk-Maxwell from his mathematical investigations of the laws of electricity. Up till the date of his work it was believed that electrical actions traveled at an infinite speed—that is to say, that a disturbance at any point in space produced its results simultaneously at all other points, however distant. Faraday had an idea that this supposition was incorrect, and tried in many ways to show that electric and magnetic actions took time to travel from place to place, but was baffled, as we now know, by the enormous speed with which he was dealing. Clerk-Maxwell, however, showed that the laws which had been proved to govern the motion of electricity indicated that an electric disturbance would travel through space with the speed of light, and that indeed light and radiant heat themselves conformed to the laws which he had proved for electric waves. For a number of years Lord Kelvin was the only scientific man of note who upheld Maxwell's view, and it was not until Hertz had experimentally demonstrated the existence and properties of electrical waves that the true significance of his remarkable deduction was realized by others.

(To be continued.)

Gas From Hardwood and Heavy Oil

The Bahia Blanca Gas Company, owing to the recent prohibition of coal exports from England, and to exces-

sive freight charges, have found it impracticable to continue making gas from coal. A process has been evolved in which hardwood is distilled in the ordinary D-retorts, maintained at a high temperature, and after the first hour, when the wood is incandescent, crude heavy oil, of an asphaltic character, is introduced at a pressure of eighty pounds per square inch, through special atomizing injectors. The resultant gas may average up to 575 B.Th.U. per cubic foot and the yield from eight tons of hardwood and 1.2 tons of heavy oil is approximately 137,700 cubic feet. The gas appears to be permanent and the wood gas is found capable of carrying the richer hydrocarbon gases from the oil.—A. M. Hunter, in *Gas Lighting*. From note in *Journal of Society of Chemical Industry*.

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